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ART. I.—*Notices of Recent Earthquakes;* by Prof. C. G. ROCKWOOD, Jr., Bowdoin College.

1. ON January 16, 1872, an earthquake almost entirely destroyed the city of Shamaka in Russia. Over one hundred persons are reported to have been killed and a large amount of property destroyed, scarcely a building having been left standing in the city. The earthquake was felt over a large extent of the surrounding country. Shamaka is a city of 25,000 inhabitants, lying at the southern base of the Caucasus Mountains, and about 75 miles west of the Caspian Sea.

2. On February 6, at 8 o'clock A. M., a slight shock of earthquake was felt at Wenona, Mich. A letter from Ed. D. Cowles of that place, states:—"The shocks were three in number and lasted altogether about thirty seconds, the vibrations travelling from the N.N.E. They jarred buildings and were plainly observable by persons out of doors, and were characterized by that peculiar rumbling sound which is noticed in subterranean vibrations."

3. On February 8, at about 5 A. M., a slight earthquake occurred at Cairo, Ill. A letter (in my possession) from Geo. Fisher of Cairo to Clinton L. Conkling of Springfield, Ill., gives the following:—"I was in bed on the second floor of a brick dwelling house. It seemed to me that something struck the head of my bed with considerable violence from the southeast, making quite a noise and shaking the entire house. The shaking continued for several seconds with varying intensity. I suppose that fully twenty seconds elapsed before it finally ceased. Persons who were up at the time seem to think that

the vibrations were from N.W. to S.E. I think they were the other way, from S.E. to N.W. No damage was done by the shock."

4. On March 6, a despatch from Berlin, Prussia, says:—"Shocks of earthquake were felt this afternoon simultaneously in Dresden, Pirna, Schandau, Chemnitz, Bodenbach, Weimar and Rudolstadt. The movement was not violent, but was more or less perceptible at intervals for over an hour."

5. On March 26, the State of California was visited by an earthquake more severe than any that has occurred there for some years.

The main shock occurred at about $2^h\ 25^m$ A. M., and was felt throughout the length of the State, from Red Bluff on the north, to San Pedro on the south, thus extending over $6\frac{1}{2}$ degrees of latitude, and from the Pacific coast inland to Virginia City, Nev.

The *time* of this shock is variously reported from $2^h\ 10^m$ at Jackson to $2^h\ 45^m$ at White Pine, Nev. The discrepancies are doubtless due partly to difference of longitude, and partly to the irregularity of watches. The most probable time seems to be that given above, which is the time reported from Visalia and Independence, where the shock was heaviest.

The *duration* of this first shock was estimated by different observers at from 2 or 3 seconds to 3 minutes. The general estimate was about one minute.

The *region shaken* was the eastern and western slopes of the Sierra Nevada, and the Sacramento, San Joaquin and Tulare valleys, extending southward even into Mexico. (A shock was reported in the City of Mexico, and in several of the Mexican States, on the same day, and presumed to be synchronous with the one in California, although the exact time was not given.) The shock was most violent, and caused the greatest damage in the Owen's river valley, Inyo Co., which is situated east of the Sierra Nevada, near to its southern extremity, and in a region covered with tokens of former volcanic action. In other places no lives were lost, and not much property destroyed.

There is great discrepancy in the reported direction of the vibrations, but in most places they appear to have been transverse to the mountain system, or in a general N.E. and S.W. direction. In Lone Pine the fallen houses seemed to have been pushed toward the N.E.

In many places the first heavy shock was followed by a series of lesser ones, closing with a stronger one at a few minutes after six A. M. And in the neighborhood of the mountains the slight shocks continued to be felt at intervals for some days or even weeks. Thus a letter from Visalia, dated April 12, says:—"Ever since the first shock of the earthquake we

have had from day to day slight shocks,—at least from three to five per day,—not sufficient to do any damage, but still somewhat exciting.*

The village of Lone Pine, eighteen miles south of Independence, in the Owen's river valley, and a place of some 500 inhabitants, was greatly damaged by the earthquake, and here was almost the only loss of life. Over fifty adobe houses were shaken into ruins, and in their fall twenty-seven persons were killed and thirty-four others seriously injured. Frame houses were shaken, but not thrown down. At Independence also many buildings were prostrated and a few lives lost.

In this valley, and at some other places, the shocks were preceded and accompanied by a loud rumbling sound, which is described as being "like a train of cars or like distant artillery."

Mention was made in the first accounts of large fissures in the ground, fires seen in the mountains, etc.; but these reports do not seem to be confirmed by the later advices. The level of Owen's lake is also said to have risen four feet.

That the shock extended under the ocean is proved by the schooner *Beal*, which was becalmed in the straits off San Pedro, and was so much injured that she made the port with difficulty. It is also worthy of mention that the volcano of Colima in Mexico was reported in violent eruption and throwing out clouds of ashes on April 4, a few days after the earthquake.

The above is gathered mostly from the numerous accounts published in the newspapers of San Francisco; and for aid in collecting them, my thanks are due to C. G. Rockwood, Esq., of Newark, N. J., J. C. Smith, Esq., secretary of the Merchants' Exchange and News Association, New York, Rev. D. W. Poor, D.D., of Oakland, Cal., and John A. Keyes, postmaster at Visalia, Cal.

It is to be hoped that more full and careful scientific accounts of the physical phenomena may have been collected by some person on the spot, and that they may in due time be given to the public.

6. A slight shock was reported at Paducah, Ky., on the morning of March 26, and another at Salt Lake City, Utah, at

* The following later news has appeared in the columns of the San Francisco Bulletin:

"*Lone Pine*, May 17, 1872.—We had such a shake to-day as we have not had since the 26th of March, when the town was reduced to ruins. There has been no cessation of the shocks, in fact, ever since. The only difference is that sometimes they are quite heavy, and again comparatively light. A remarkable fact, however, is that the shocks are no longer long, and like a sea swell, but are short, sharp, and appear to describe a circle. I was sitting in the parlor of my house to-day when the shock came, about one o'clock. It had been preceded by several lighter agitations, accompanied by the inevitable rumbling sound. Every timber in the house shook, the furniture in the room was violently disturbed, and I was actually thrown from my seat. The shock lasted some thirty seconds."

1 P. M., March 28. These may have been part of the Inyo earthquake.

7. On April 3, at 8 o'clock A. M., a severe earthquake destroyed a large part of the ancient city of Antioch in Syria. The shock lasted over 40 seconds, and the wave travelled from east to west. It was accompanied by a noise "like distant thunder or artillery." Lighter shocks continued to be felt at irregular intervals for at least a week after the first one. Very many buildings were shaken down, filling the narrow streets with the débris and burying hundreds of the inhabitants beneath the ruins. The number of killed is estimated at 1,000 to 1,600 and many more were left without shelter. The old Roman bridge of four arches is rent in several places. The villages south of the Orontes river were also much injured; but comparatively little harm was done north of the city.

8. A despatch from Copenhagen, May 14, gives the following:—"A schooner, which arrived here to-day from Iceland, reports a series of violent earthquake shocks at Hasvick on the 16th, 17th and 18th of April. Twenty houses were destroyed, and several persons were injured, but no lives were lost."

9. The recent grand eruption of Mt. Vesuvius is interesting, as being possibly connected with the phenomena recorded above. This eruption first assumed noticeable proportions on the night of April 24, 1872, when a flow of lava was added to the flames and smoke which had for months adorned the summit of the mountain. On the night of the 25th, a chasm opened in the side of the cone, from which issued a torrent of lava; the whole occurring so suddenly as to overtake and destroy a number of the spectators who were watching the eruption. The flow of lava continued two or three days, overwhelmed two villages, and buried a considerable extent of cultivated land. The eruption finally ended with a shower of stones and volcanic sand, which fell in the streets of Naples to the depth of several inches. The eruption was attended with the usual local tremblings of the earth.

Brunswick, Me., May 31, 1872.

ART. II.—*Contributions from the Physical Laboratory of Harvard College;* No. III. *On the Electrical Condition of Gas Flames;* by JOHN TROWBRIDGE, Assistant Professor of Physics.

PROF. H. BUFF, of the University of Giessen, has published in the Annalen der Chemie und Pharmacie, vol. lxxx, 1, and in the Phil. Mag. of Feb., 1852, an investigation of the electrical properties of flames. He reviews at first the different

theories in regard to the subject; Becquerel, for instance, finds electric opposition in all directions in flames which depend upon the difference of the temperature of the metals immersed in them. Pouillet recognizes a motion of electricity only from the interior to the exterior, and hence also from the base to the summit of the flame; Hankel, however, finds a motion the reverse of this in the flames produced by the ignition of spirits, and states that it is independent of the temperature of the immersed conductor.

Prof. Buff then gives the following as the results of his investigation:

1. Gaseous bodies which have been rendered conductible by strong heating are capable of exciting other conductors, solid as well as gaseous, electrically.

2. When a thermo-electric circuit is formed of air, hydrogen or carburetted hydrogen, alcohol vapor, charcoal, or finally a metal, whether combustible or incombustible, an electric current is developed, which proceeds through the air from the hottest place of contact to the less warm place.

3. The development of electricity which has been observed in processes of combustion, and particularly in flames, is due to thermo-electric excitation, and stands in no immediate connection with the chemical process.

4. The products of combustion do not therefore, by any means, occupy the relation to the burning body which has been assumed by Pouillet; if positive electricity rises with the ascending gases, it is only in the degree in which the air exterior to the place of hottest contact is connected by a proper conductor.

The following are the results which I have obtained in testing the electrical condition of the flame of a Bunsen burner with a Sir William Thomson's quadrant electrometer. The degrees given refer to the arbitrary divisions of the scale, upon which a spot of light is reflected from the mirror of the instrument.

Upon connecting the testing plate of one pair of quadrants of the instrument with the flame, while the other pair were connected with the metallic burner and with the earth, the flame was found to be negatively electrified.

The following are some of the experiments selected from a series that were made.

Exp. 1. Flame 12 c. m. high; plate at the height of 7 c. m. A negative indication of 130°, very steady.

Exp. 2. A platinum wire, substituted for the plate, and meeting the flame 3 c. m. above the burner, gave a deflection of 30° in a negative direction.

Exp. 3. With the testing plate just above the tip of the

flame, the instrument showed a positive deflection of 70 to 80 degrees.

Exp. 4. With the testing plate 5 mm. from the outer surface of the flame, on all sides, a feeble positive charge was obtained, the air in contact with the flame being apparently charged positively, the indication in no case exceeding fifty or sixty degrees on the scale of the electrometer.

Exp. 5. The metallic tip of the burner was found to be charged positively, giving an indication closely agreeing in the number of degrees with that corresponding to the negative indication of the flame. This indication was quite constant.

Exp. 6. When a glass tip was substituted for the metallic tip, no charge was found upon it. This was the case when any non-conducting body formed the tip.

Exp. 7. A glass tip having been substituted for the metallic one, a platinum wire was inserted below the orifice and carefully pushed upward until it occupied the centre of the interior cone of flame. A very feeble indication of negative electricity was the result.

While, with the Bunsen burner, the flame and the metallic tip are in decided electrical opposition; the one having a negative charge and the other a nearly equal positive charge; in spirit flames the two opposite states recombine, the wick of the lamp and the fluid contained in the vessel connecting the two charges. The flame, therefore, merely takes the potential of the atmospheric electricity at the place where it is situated.

The electrical condition of the flame of a Bunsen burner when tested by a sensitive galvanometer gives in the main the same results as those obtained by Prof. Buff from spirit flames. The quantity of electricity in the current passing from the flame to the tip is exceedingly small; whereas we have seen above that the terminal immersed in the gas flame has a tension a little exceeding that of the negative pole of a Daniell's element.

The air in the room, at the time the above experiments were first performed, was charged positively to about the tension of the positive pole of twelve Daniell's elements. The experiments were afterward repeated when the air in the room indicated a negative charge, with no difference in the results.

At the suggestion of Dr. Wolcott Gibbs, Rumford professor, I tried the above experiments with a Bunsen blast lamp, by means of which I could increase the heat and the flow of air and gas at pleasure. Slight deviations in the scale readings were obtained by this means: the flow of air appeared to affect the charge of electricity upon the metallic tip, rendering it less constant. The above experiments were in the main confirmed. The nature of the metallic plate submitted to the flame and

the degree to which it was heated appeared to have a very slight influence upon the charge.

Sir William Thomson, in the proceedings of the Literary and Philosophical Society of Manchester, March, 1862, has a paper upon the electricity of the air in rooms. He finds that it is generally negative. By placing a spirit lamp upon the prime conductor of an electrical machine, he was enabled to change the tension of the air from a positive to a negative state and the reverse. He carefully separates the results obtained from the *idioelectric* effect of the flame, which, he states, in no case gave a tension equal to either pole of a Daniell's element.

During the past winter observations made in the laboratory tend to confirm these views. I have, however, found on some days the air within strongly positive. The room is in the north-west corner of the building, and there was a strong north-west wind blowing at the times this was observed. I noticed, also, while experimenting with the flame of a Bunsen burner placed near the water dropper used by Sir William Thomson in investigating the electrical state of the atmosphere, that the positive charge of the air in the neighborhood greatly decreased, and in some instances became feebly negative, by the presence of the flame.

The popular idea that great fires are followed by a change in the atmosphere inducing rain, does not seem to be unwarranted from an electrical point of view. The electricity of the air during cloudy and rainy weather is generally negative or at the most feebly positive. The flames being negative would tend to change a strong positive charge in clear weather to a negative one, and thus bring the air to the state noticed in cloudy and rainy weather.

The following are the conclusions to which the above experiments lead:

1. The flame of a Bunson burner is negative while positive electricity accumulates on the burner itself, if it is a good conductor. With orifices made of non-conductors, no charge was found upon the tip.

2. The stratum of air in contact with the outer cone of flame is slightly charged with positive electricity. The partly consumed gas of the interior cone is neutral.

3. The presence of flames tends to change the nature of the atmospheric electricity at the given place, reducing a positive tension to a feebly negative one.

Art. III.—*On Molecular and Cosmical Physics;* by Prof. W. A. Norton.

[Concluded.]

13. If we include in the curve of effective molecular action the external electric attraction which we have seen may arise within the sphere of what has been called the effective external repulsion, it will be seen that the curve beyond *Oc* should be raised; and that when an effective attraction supervenes, and determines a chemical union, it will pass entirely above the zero line, from *c* to a certain distance beyond *Od*. But instead of this the result may be that it passes above the zero line for a certain distance, beginning at a point, say *a'*, beyond *c*, or even *d*. That is, an effective external repulsion may be in operation over a distance *ca'*, and be succeeded by an *external attraction* for a certain distance beyond *a'*; and accordingly *a'* will be a second point of stable equilibrium. Such an external effective attraction, succeeded at less distances, down to *c*, by a repulsion, is apparently the determining cause of all phenomena that result from bodies of matter coming into contact, which do not terminate in a chemical union. As this attraction operates through a certain distance before the equilibrium is established, more or less heat is generally given out.

We recognize it (1) as the force of *adhesive attraction* between solids and liquids (which experiments have shown to be very feeble in comparison with the cohesive attraction that constitutes the general force of tenacity of the molecules of the liquid), of which capillary attraction is one mode of manifestation: (2) as the similar force by which gases are compressed on the surfaces of solids and liquids, or into their pores, with the evolution of heat: (3) as the operative cause of the diverse phenomena of *liquid* and *gaseous diffusion*. To understand how it is capable of producing such effects, we have only to reflect that at the surface of contact of two liquids, or gases, the force in question should come into operation between the dissimilar molecules posited on opposite sides of the surface, and that the result must be that the molecular repulsion exerted by the similar molecules of either liquid or gas, on one side of the surface, will urge those lying near it to the opposite side. This should happen, in the case of a gas, even though the force under consideration should not amount to an effective attraction, but should only diminish the repulsion between the dissimilar molecules. In this fact we see the reason why all gases interpenetrate each other. The diffusion should not stop until the two gases become a uniform mixture, since as long as an ideal plane can be taken within the gaseous mass,

on opposite sides of which the number of molecules of the two kinds is not the same, the repulsion that takes effect across this plane will be of a less intensity than that which is directed toward it.

Since the force of electric attraction that comes into play between two dissimilar gaseous molecules, lying on opposite sides of the surface of contact, is of the same intensity for each molecule, and the molecular repulsion of each gas is the same, the velocity of diffusion of each should be inversely proportional to the square root of the density; which is the fundamental law of gaseous diffusion established by Professor Graham. In the case of two liquids the result is not precisely the same, since the molecular repulsion may be of different intensities for the two liquids, and hence the moving forces of the two dissimilar molecules may be unequal. This repulsion is generally least for the more viscid liquid. The general result, however, is, as in the case of a gas, that the fluid whose specific gravity is the least, penetrates most rapidly into the other.

It may be added that the external attraction under consideration is also the force which gives to liquids their solvent power.

14. When two substances combine in several different proportions, the force of affinity is ordinarily weaker in proportion as the number of primitive molecules ("atoms") of the one that combine with one of the other is greater. This may be ascribed to the circumstance that each new combination withdraws a portion of the electric ether from the sides of the molecules on which union has previously taken place, and occasions a collapse of the envelopes there. The result of this should generally be that the molecular attraction subsisting there is weakened, and that developed at the new point is less than that before existing. The point of saturation should be reached when the diminishing attractions have become so feeble that the heat developed by a new combination suffices to detach one of the atoms already combined; or when the decreasing force of external attraction, that comes into operation at the successive combinations, is not sufficient to predominate over the external repulsion.

There should be a tendency, in any case of a chemical union of the combining molecules, to modify the electric condition of those in their vicinity, by a propagation of the disturbance of the envelopes. In view of this tendency we may see how it is that a minute change in the proportion of one of the constituents may effect a marked change in the degree of tenacity, hardness, &c., of the compound (e. g., different qualities of steel resulting from small differences in the quantity of carbon that is combined with the iron). The phenomena of fermentation may be referred to the same cause.

15. All the mechanical properties of bodies may be ascribed to varied values of the quantities $\frac{n}{m}$, m, and r^2 (vol. iii, p. 338), and to the variations that may occur in these values under different circumstances. These quantities must depend primarily upon the mass and size of the atoms around which the ethereal atmospheres and electric envelopes are condensed. The marked difference in the properties of certain substances which have nearly the same atomic weight, indicates that atoms of the same mass may differ in size.

It is to be observed that the same substance may assume various states of aggregation of its primitive molecules, in which it exhibits different properties; under varied thermal or other circumstances of solidification, giving rise to modifications of the molecular envelopes. One general result may be noted, viz., that an aggregation of compound molecules should have less tenacity than one of primitive molecules of the same substance; since in the latter case no two molecules can be drawn asunder without exerting a force equal in intensity to their maximum effective attraction; but if the primitive molecules are arranged in groups, instead of one homogeneous mass, there must be a number of the constituents of each group separated from those of the contiguous one by a distance greater than the natural distance between two primitive molecules in equilibrium. Diamond and charcoal may be cited as examples of the two states of aggregation under consideration.

16. When the molecules of a solid, or liquid, are in a state of equilibrium, that portion of the heat-pulses which acts as a molecular repulsion is insensible heat; since all the repulsive impulses that proceed from one primitive molecule and take effect on another, are there neutralized by equal attractive impulses. Whenever, in the disturbance of the equilibrium of two contiguous molecules, heat is given out, a certain part of these insensible impulses passes into the sensible state; that is, are propagated indefinitely as unneutralized heat-pulses. At the same time a change generally occurs in the physical condition of the molecules; ordinarily their envelopes collapse.

17. Heat may originate in several different ways. (1) When molecules are forced, or in any way made to come into closer proximity, in opposition to an effective repulsion, the entire amount of the repulsive impulses that operate as a retarding force, shows itself as sensible heat. Heat originates in this manner in the act of chemical union, in which a certain amount of effective molecular attraction is expended in urging the molecules toward each other, and an equal amount of effective repulsion destroys the motion, and in so doing originates ethereal waves of heat proceeding outward from the

molecules, which convey impulses of the same intensity as those expended in arresting the molecular movements. Heat is developed in this way when bodies are compressed by pressure, or impact. The heat of friction also originates in this manner; for between the molecules of the two rubbing surfaces the force of external repulsion is in operation, and certain pairs of them are being incessantly urged nearer to each other on the line of movement. The increasing repulsion thus developed constitutes the force of friction, and the resisting work done by it develops an equivalent heat-energy. It may seem that the continued operation of the same repulsive force, after the approaching particles have passed each other, should counteract the effects resulting from their approach; but it is to be observed with regard to the effect of this circumstance, that any work thus done by the molecular heat-repulsion operating as an accelerating force will be given out again, by reason of the subsidence of the motion produced by it, and therefore whatever heat-energy is expended in such motion is afterward restored. Besides, the repulsion in operation between two surface molecules, in the act of separating after having been forced into close proximity, will in general be more inclined to the line of movement than the repulsion exerted between the same molecules during their approach. This inequality of inclination should obviously increase with the pressure.

(2.) Heat may also be developed by reason of a collapse of molecular envelopes, resulting from a change in the distance between contiguous molecules. For example, all substances for which the ratio $\frac{n}{m}$ exceeds 6·9, will, when compressed, give out heat in this manner; since the mutual attraction of their envelopes will diminish. We have seen that the same state of things occurs with india-rubber when it is stretched. In fact, it appears from the table on page 444 (vol. iii), that all bodies of matter for which the ratio $\frac{n}{m}$ is less than 6·9 are in this respect in the same condition with india-rubber. There are certain theoretical reasons for believing that all liquids belong to this class. Upon this view the heat of congelation may be ascribed to a collapse of the molecular envelopes resulting from the expansion of the mass that ensues as the compressive force at the surface of the liquid ceases to operate. Except in the case of water, the molecules of the liquid being in what has been termed the secondary condition, this collapse of the envelopes will be attended with an augmentation of their attractive actions and a consequent attraction of the mass; but this augmentation will increase the ratio $\frac{n}{m}$, and so tend to make the

expansion of the envelopes that should result from the contraction of the mass less than the previous contraction. Besides, the operation of the increased attraction should tend directly to develope sensible heat-pulses for the reason given above.

(3.) The evolution of heat may also result from the action of an electric current,—either compressing the molecular envelopes, and thereby causing heat-pulses to proceed from the ethereal atmospheres, or originating waves in the interstitial luminiferous ether, the energy of which is subsequently absorbed by the molecular envelopes, to be radiated off in heat-pulses.

18. All the diverse effective forces in operation, in or upon bodies, may act as *statical forces*, or *dynamical forces*. In the first case the impulses that fall upon the central atoms of the primitive molecules are reflected off again, either in electric or ethereal waves; and no transformation of motion from ethereal to ordinary matter occurs. But when motion ensues a certain amount of ethereal wave-force is expended in the production of such motion, and is transformed into the equivalent dynamic energy of the moving mass. This motion can only be counteracted, and the body brought to rest, by the exertion, either directly or indirectly, of an amount of impulsive wave-force equal to that which originated the motion; in which event the energy that had passed into the body is restored to the ether, either luminiferous or electric—ordinarily the former in the form of heat-waves, since the repulsion that operates to bring an impinging body to rest is usually the molecular heat-repulsion taking effect at the surface, and in the interior of the body

The dynamical energy of any moving mass, of either of the three varieties of matter, represents the previous expenditure of a certain amount of ethereal wave-force, and can disappear only by being transformed into an equivalent amount of energy of one of the other two varieties of matter. Thus radiant heat is a dynamical energy that has resulted from the expenditure of a certain amount of the molecular heat-repulsion before noticed; and may be transformed into an equivalent amount of energy of ordinary matter, or of electric ether.

19. Heat is everywhere in a state of perpetual flux,—both while it retains the condition of ethereal wave-force and while in the act of transformation into an equivalent energy of either of the other two forms of matter; and each transformation ordinarily soon gives place to another, and so never ending cycles of transformation are passed through.

When the pulses of radiant heat fall upon ordinary molecules one portion of the wave-force is transmitted, another reflected, and a third absorbed. *Absorption* is of two kinds, *general* and

elective. That portion of heat-energy which experiences general absorption, is consumed in urging the molecular envelopes farther from the central nuclei, while the other is expended in imparting vibratory movements to the atoms and layers of the envelopes, which naturally vibrate in unison with especial heat-waves. *Radiation* is the necessary correlative of absorption. For the recess of the envelopes attendant upon absorption is succeeded by an equal collapse that originates pulses of radiant heat equal in amount to those absorbed; and the special vibratory movements induced subside again by being communicated to the luminiferous ether. The collapse of an envelope is attended with vibratory movements of its individual atoms both toward and from the central nucleus, and at right angles to this line (varying in rate with the position of the atom in the envelope), which originate ethereal waves of diverse rates of undulation.

Heat is *conducted*, by good conductors, chiefly by waves of the electric ether passing over from one molecular envelope to another; and hence the same physical conditions which favor the conduction of heat should also favor the conduction of electricity. Heat may also be slowly and imperfectly conducted by successive radiations from molecule to molecule through the luminiferous ether, with attendant absorptions by the molecular envelopes; when the density of the interstitial electric ether is too feeble to admit of direct conduction.

20. What is called the interior potential energy of a body of matter, in the mechanical theory of heat, is the mechanical or equivalent thermal energy capable of being expended, or given out, in a contraction of the mass after expansion, or in the condensation of simple into compound molecules, or in the collapse of expanded molecular envelopes. When the expansion of envelopes has resulted immediately from an increased attraction between contiguous molecular envelopes, superinduced by an extraneous action (mechanical or thermal), the potential heat-energy absorbed will be equivalent to the attractive energy expended. The work done, if any, in this case by the extraneous action in opposition to molecular resistances is altogether distinct from this incidental effect. The mechanical stretching of wires, and liquefaction, with the attendant absorption of heat, may be cited as illustrative examples.

The hypothesis now in vogue that heat imparts vibratory movements to the atoms themselves of bodies, involves the assumption that the vibrations are as rapid, or approximately so, as the undulations of the heat-waves; which the comparatively slow rate of transmission of sound-vibrations, and the comparatively feeble intensities of the elastic forces called into play in their transmission, renders in the highest degree improbable.

21. Waves of light and of the actinic force originate, like those of heat, in the vibratory movements of the atoms, or layers, of the electric envelopes of molecules.

22. The diverse properties of substances with respect to the absorption and radiation of special rays of light and heat, as revealed by the *spectroscope*, may be chiefly ascribed to diversities in the range and rate of variation of the intensity of the forces acting on the atoms of the electric envelopes of molecules. The envelope of the molecule of each substance is in a special condition of equilibrium, both as a whole and in all its parts; and any displacement of it, by heat-waves for example, will originate special rates and systems of vibration throughout its mass. It will in general vibrate as a whole, or in subdivisions of greater or less extent, or in its individual atoms. Stationary waves, and nodal points, lines, or surfaces, may also supervene by the combination of individual waves passing up and down through the envelope, or to and fro around it. In fact we have all the conditions necessary for the production of the diverse systems of bright or dark lines noticed in the spectra obtained with different substances, and also the continuous spectrum formed under certain circumstances. The commingling of initial waves with the direct and reflex waves from other contiguous molecular envelopes may also play a certain part in the phenomena.

Physicists have sought for a similar theoretical result by conceiving that the molecules of the vapor or gas are compound; but the fact that each incandescent gas when sufficiently compressed gives a continuous spectrum, necessitates the supposition that the gaseous molecule is composed of an indefinitely great number of simple molecules, which cannot be admitted. This conclusion cannot be avoided if we allow that to each rate of undulation of a ray belongs a particular degree of refrangibility.

23. The principle of the "Correlation of Forces," is implied in the doctrine of convertible energy that has been briefly set forth (p. 12). It applies to the dynamic energies that have resulted from the operation of cosmical or molecular forces during certain previous intervals of time. These energies are over and above the forces attendant at any instant upon the natural statical condition or tendency of things; and hence are so many disturbances of the natural equilibrium, and as this always tends to assert itself, must continually manifest themselves in connection with material movements, and these movements must be continually undergoing transformations, wherever the moving masses (whether of either of the two ethers or of ordinary matter) come into contact with others at rest.

24. In the gaseous state of matter the only molecular force in

operation is the heat-repulsion—the mutual attractive actions of the molecular envelopes having become insensible. The *law of Mariotte* may be deduced from this theoretical principle in the following manner. Let *m* be a point in the enclosure against which the elastic pressure is exerted, and *cmd* a slightly divergent cone; all the gaseous particles lying within this cone will be the centers of heat-waves proceeding in all directions, and in the thermal equilibrium of the mass each molecule will radiate an amount of heat equal to that which it receives by absorption from surrounding molecules. There will accordingly be a uniform wave-flow of heat from one layer *ab* of the conical mass to the next toward *m*, and thus the quantity of wave-force that falls on *m* is the same as if there were an uninterrupted flow directly from one layer *ab*. Now if the density of the gas be doubled, the number of molecules, or wave-centers in any layer *ab* will be doubled, and hence the wave-force impinging on *m* will be doubled. The result is the same as if, for every such slightly divergent cone, there was a line of aerial particles moving with a certain uniform velocity and impinging upon *m*; the density of this representative line being proportional to the density of the gas. This representative idea accords with the fundamental hypothesis of the kinetic theory of gases. The known deviations from the strict law of the proportionality of the elastic pressure to the density may be ascribed to the fact that the mutual attractive action of the molecular envelopes becomes sensible when the density is greatly increased, and the distance between the molecules approximates to *Od* (fig. 4, vol. iii, p. 339). The well known experiment by Joule, which established that a gas expanding into a vacuum experiences no change of temperature, shows that the heat-energy lost in the expansion of the escaping gas is restored by the impact of the particles upon the enclosure. This should obviously be the result if, as we have supposed, gaseous phenomena are entirely due to the operation of molecular heat-repulsion, since the energy of the repulsive heat-waves expended in imparting velocity to the particles should be given out again when the motion of the particles is arrested.

5.

It is obvious from the explanation above given of the law of Mariotte that if two different gases have the same temperature they will exert the same elastic outward pressure, provided the number of their molecules is the same for equal volumes; or, in other words, at the same temperature and pressure the number of particles in equal volumes of the two gases should be the same.

The specific heat of different gases should be the same under

similar circumstances, unless some portion of the sensible heat applied should be expended in doing work within the molecules, and portions so expended should be unequal. Now in the case of simple gaseous molecules heat can do such interior work only by expanding their envelopes, and there is no apparent reason for any such possible effects being different with different gases. The case is different with compound molecules, for the expansive action of the heat will in general tend to develope an increased attraction between the envelopes of the constituents of the compound molecules, the operation of which will give rise to an absorption of a certain amount of heat. Such effects may be quite different for different compound gases; and so their specific heats may be quite unequal. The specific heats of compound gases, we should at the same time expect, would be greater than that of a simple gas.

ART. IV.—*On the Datolite from Bergen Hill, New Jersey;* by EDWARD S. DANA. With Plate I.

THE Bergen Hill tunnel is famous for the abundance, beauty, and variety of the minerals which it brought to light. Datolite, pectolite, calcite, analcite, apophyllite, natrolite, stilbite, and others were obtained there during its excavation in a degree of perfection rarely equaled by the productions of any other locality. The crystallizations of datolite are especially remarkable; some of the surfaces covered with the brilliant crystals being eighteen to twenty-four inches in length.*

The crystals are in general not over a third of an inch across, though they sometimes have a diameter of one inch. Those of a single specimen have always entire uniformity of habit. The datolite is associated on different specimens with most of the other species found at the same locality, but it was not found possible to obtain any facts which would throw light upon the influence of the associated minerals on the crystalline form.

Among the varied forms, four different types may be distinguished.

a. Figure 1 represents a very common and characteristic form. The crystals here are very thin, wedge-like, and are attached to the mass of rock approximately by the extremity of the clinodiagonal, though varying from that of the diagonal terminating between 2 and 2 on one side, to that between −4 and −4 on the other. From the position and shape of these crystals, they offer an unusual number of surfaces for the reflection of the light, and hence give the specimen a brilliant sparkling aspect,

* Many specimens of this kind occur in the collection of Mr. Haines of Elizabeth, N. J.

which serves to distinguish them at a glance from all other forms. It is always simple, no further modifications than those figured having been observed.

b. Another and more interesting type is shown in figure 2. The crystals here are approximately equal in the three dimensions. The figure shows this form in its simplest condition. Other planes occur, viz: $-2\text{-}i$; also (as in fig. 3, which is a portion of a crystal with this habit) $2\text{-}i$, 4-2, $\frac{4}{3}$, $\frac{4}{5}$, $\frac{2}{3}$, $\frac{4}{3}\text{-}\grave{\imath}$, $-8\text{-}\grave{2}$; these modifications do not however change at all the general habit of the crystals.

c. Figure 14 represents a rare and quite unique form. The planes $-2\text{-}i$ and 2 are here most prominent, and when the *I* becomes merely a line and $i\text{-}i$ a minute triangle, as is sometimes the case, it has the aspect of a rhombohedron. The planes $i\text{-}2$ and $\frac{9}{4}\text{-}\grave{9}$ are noticeable features of the figure, and are spoken of more particularly in another place. In addition to the planes figured, there occur on crystals of the same specimen, $\grave{1}$ very small, and $i\text{-}\grave{\imath}$, $i\text{-}\grave{2}$, $2\text{-}i$ as mere lines.

d. Another type of form, though not so distinct, is exhibited in figure 13. Under this type we seldom find the crystals of two specimens exactly similar, there being a great variation in the relative sizes and in the number of the planes. A very complex form of this type is represented in fig. 8. The special points to be noted in crystals of this habit are: the plane $-2\text{-}i$, which is often very large and almost invariably etched and therefore without polish; and also the presence of the series $-4\text{-}2$, $-4\text{-}4$, $-4\text{-}i$ and $-6\text{-}\frac{3}{2}$, $-6\text{-}3$, $-6\text{-}i$ (fig. 4). The planes of the series last mentioned are very common, the two series frequently occurring together; and one of them at least is almost always present with the $-2\text{-}i$. This peculiar range of planes above $i\text{-}i$ does not appear to have been met with on crystals from foreign localities.

Under these four distinct types and their modifications, all the crystals of Bergen Hill datolite observed by the writer (on over two hundred specimens) can be included.

The following is a list of all the planes observed; in it, those marked with an asterisk (*) have not been observed before: *O* (top); vertical prisms, $i\text{-}i$, $i\text{-}4$,* $i\text{-}2$,* *I*, $i\text{-}\frac{3}{2}$, $i\text{-}\grave{2}$, $i\text{-}\grave{3}$,* $i\text{-}\grave{\imath}$; orthodomes, $-1\text{-}i$, $-\frac{4}{3}\text{-}i$, $-2\text{-}i$, $-4\text{-}i$, $-6\text{-}i$, $-8\text{-}i$; $2\text{-}i$; clinodomes, $\frac{1}{2}\text{-}\grave{\imath}$,* $1\text{-}\grave{\imath}$, $\frac{4}{3}\text{-}\grave{\imath}$, $2\text{-}\grave{\imath}$, $4\text{-}\grave{\imath}$; hemi-octahedral, -4; 4, 2, $\frac{4}{3}$, 1, $\frac{4}{3}$, $\frac{2}{3}$;* $-4\text{-}2$, $-4\text{-}4$,* $-6\text{-}\frac{3}{2}$,* $-6\text{-}3$; $4\text{-}2$;* $-6\text{-}\grave{\frac{3}{2}}$* (?), $12\text{-}\grave{\frac{3}{2}}$; $-4\text{-}\grave{2}$, $-8\text{-}\grave{2}$, $\frac{8}{3}\text{-}\grave{2}$, $2\text{-}\grave{2}$; $-4\text{-}\grave{3}$,* $\frac{4}{3}\text{-}\grave{3}$,* $\frac{3}{2}\text{-}\grave{3}$;* $\frac{9}{4}\text{-}\grave{9}$,* $\frac{5}{2}\text{-}\grave{5}$,* $\frac{8}{7}\text{-}\grave{4}$* (?), $\frac{8}{9}\text{-}\grave{\frac{4}{3}}$* (?).†

† The plane $6\text{-}i$, found by Hessenberg (Min. Not., IV, where it is designated $\frac{2}{3}$P∞) on a crystal from Bergen Hill, I have not observed except on crystals from Roaring Brook, Conn.

Among the new planes, the following are determined by the zones in which they occur, as will be seen in the figures: $-6\text{-}\frac{3}{2}$, $-4\text{-}4$ (fig. 4), $4\text{-}2$ (fig. 3), $i\text{-}2$ (fig. 14), $-4\text{-}\breve{3}$ (fig. 8,) $\frac{3}{2}\text{-}\breve{3}$ (fig. 12),* $\frac{1}{2}\text{-}\grave{\imath}$ (fig. 10, between 1 and $1\text{-}\grave{\imath}$). $O \wedge \frac{1}{2}\text{-}\grave{\imath} = 171°\ 1'$ (measured, 171° 7′). The plane $i\text{-}2$ consists of an oscillatory combination of $i\text{-}i$ and I.

Other new vertical prisms are $i\text{-}4$ (fig. 11), and $i\text{-}\breve{3}$ (fig. 8); $i\text{-}4 \wedge i\text{-}i = 170°\ 58'$ (measured = 171° 11′); $i\text{-}\breve{3} \wedge i\text{-}i = 117°\ 39'$ (measured = 117° 51′). $\frac{2}{3}$ (fig. 3) is a new octahedral plane; $O \wedge \frac{2}{3} = 158°\ 36'$ (measured = 158° 50′). $\frac{4}{3}\text{-}\breve{3}$ has always the shape and position shown in figs. 9 and 12; $i\text{-}i \wedge \frac{4}{3}\text{-}\breve{3} = 108°4'$ (measured = 107°–108°); $O \wedge \frac{4}{3}\text{-}\breve{3} = 138°\ 14'$ (measured = 138°). $-6\text{-}\frac{3}{2}$ (fig. 9) is on the edge between I and $-4\text{-}\breve{2}$, consequently $m = \frac{2n}{n-1}$; the case did not admit of determination by measurement; but n is obviously less than 2 and greater than 1, and it is hence very probably equal to $\frac{3}{2}$, which puts it in the same zone with $12\text{-}\frac{3}{2}$.

Between $\frac{4}{3}$ and $2\text{-}\grave{\imath}$ a rough plane was observed in one case. Here $m = \frac{4n}{2n+1}$; the plane gives an angle of 165° to 168° upon $2\text{-}\grave{\imath}$, and consequently n cannot be less than 3.

A remarkable series of planes, usually convex (figs. 16, 17), in the same zone with $-2\text{-}i$, $2\text{-}\grave{\imath}$ and I (opposite $-2\text{-}i$) having $m = \frac{2n}{n-1}$, often takes the place of the clinodome $2\text{-}\grave{\imath}$, which when present is very narrow. The common form of this $+m\text{-}\breve{n}$ plane (G) is convex; starting from $-2\text{-}i$, where it makes on $i\text{-}i$ an angle of about 95°, it curves around toward I, changing in the value of m and n constantly till the intersections with $4\text{-}\grave{\imath}$ and 1 become parallel and it makes an angle on $i\text{-}i$ of 101° to 102° 30′. This is represented in fig. 17, which is a front view of the plane. In fig. 14 we have a plane of this zone quite flat though unpolished, and giving on measurement the angle on $i\text{-}i$ of 94° 30′–96°; its symbol is consequently $\frac{2}{1}\text{-}\breve{9}$ (required 95° 42′). Another plane of this zone, also sometimes occurring alone, as shown in fig. 16, a direct view of it, makes parallel intersections with $4\text{-}\grave{\imath}$ and 1, and is hence $\frac{4}{3}\text{-}\breve{5}$; $i\text{-}i \wedge \frac{4}{3}\text{-}\breve{5} = 100°\ 57'$. On the edge between $\frac{4}{3}\text{-}\breve{5}$ and $-2\text{-}i$ in the same crystal, a plane appears (ν, fig. 16) which corresponds to the other part of G.

Des Cloizeaux gives the planes γ, η, χ, ω, ϑ, misapprehending the form which he takes from Dana's Mineralogy; placed in the proper position γ would become $d\frac{1}{2}$, χ, q, etc. γ ($-2\text{-}2$), however, he figures also from a crystal of Haytorite.

* In figures 12 and 15 the crystal is represented inverted, which was necessary in order to show well the new planes.

The clinodome $\frac{4}{3}$-$\grave{i}$ is also occasionally wholly or in part replaced by a convex $+m$-$\grave{n}$ plane (τ), in the zone that includes 4, $\frac{4}{3}$-$\grave{i}$ and $\frac{4}{3}$, which plane therefore has $m=\frac{4n}{2+3n}$. In fig. 6 the plane (τ) occurs, though by the projection its shape is not satisfactorily shown. Measurement gives for $\tau \wedge i$-i=91° 30′ to 92° 30′ near its intersection with -2-i; but for the larger part of the plane $\tau \wedge i$-i=97° –98°, and near its intersection with $\frac{4}{3}$, $\tau \wedge i$-i= 107° 20′–108°. The last angle corresponds to the plane $\frac{8}{9}$-$\frac{4}{3}$, whose calculated inclination on i-i is 107° 36′; while the main part of the plane has for n probably 4, giving $\frac{8}{7}$-$\grave{4}$, which inclines toward i-i at the angle 97° 31′. There may be included in τ the plane $\frac{8}{5}$-$\grave{6}$, for which the same angle is 95° 13′. The plane 1-$\grave{2}$ belongs to the same zone; but its angle with i-$\grave{i}$ is 103° 36′, and evidence of its presence was not found, while that of $\frac{8}{9}$-$\frac{4}{3}$ was sustained by the following observation.

Between 1 and -2-i a convex plane (η fig. 15) occurs, giving i-$i \wedge \eta$=105°–107°; from the zone we have $m=\frac{2n}{3n-1}$. This plane is in a zone between 1 and the position of $\frac{8}{5}$-$\grave{i}$, while $\frac{8}{9}$-$\frac{4}{3}$ is in that between $\frac{4}{3}$ and $\frac{4}{3}$-$\grave{i}$. The two zones here cross, and if η is the plane common to them, it is $\frac{8}{9}$-$\frac{4}{3}$. That it is so is rendered almost certain by its angle of inclination on i-i, and its similarity to τ in having a convex surface.

The convex planes G and τ replacing the clinodomes 2-$\grave{i}$ and $\frac{4}{3}$-$\grave{i}$, sometimes occur together. In fig. 6, the intersections of O with the two planes τ converge backward, a necessary consequence of the oblique position of τ.

The series of planes above i-i, -4-i and -6-i and the adjoining planes, are in many cases curved, so that sometimes all intersections between them disappear; this is true of -6-i and -8-i in fig. 6. These series of curved planes are a remarkable feature of the datolite from this locality.

In regard to the special character of different planes, and their frequency of occurrence, I make a few additional remarks.

The clinodiagonal hemi-octahedral planes and vertical prisms, almost without exception, are destitute of any polish, and often quite rough; this is true also of -2-i and the other orthodomes with -6-3 -6-$\frac{3}{2}$ -4-4, 4-2, and 4. The remaining planes, with occasional exceptions, are well polished, though the presence of wavy lines on the surface in most cases prevents very accurate measurements. O, when of sufficient size to be well observed, is uniformly striated in two different directions, parallel to its intersections with the octahedral planes of the m series. In the crystals from which fig. 3 was drawn, it consisted of an oscilla-

tory combination of *O* and $\frac{2}{3}$, introducing so much irregularity as to make $\frac{4}{3}$-$\grave{i}$ at times slightly triangular in shape.

The planes *I*, *i*-*i*, 4-$\grave{i}$, −4, and 2 are never absent; −2-*i*, 2-$\grave{i}$, *O*, 1 are very common; $\frac{4}{3}$-$\grave{i}$, −4-*i*, −6-*i*, −4-2, −4-4, −6-$\frac{3}{2}$, −6-3, a little less common; *i*-$\frac{3}{2}$, *i*-2, −8-2, 4, (the last generally associated with the clinodiagonal planes), were observed on perhaps one-tenth of the specimens examined; −1-*i*, −$\frac{4}{3}$-*i*, $\frac{4}{3}$, $\frac{4}{3}$, 2-*i*, 12-$\frac{3}{2}$, $\frac{9}{4}$-9, $\frac{8}{5}$-$\frac{4}{3}$, on one-twentieth; −8-*i*, *i*-2, 4-2, $\frac{2}{3}$, $\frac{1}{2}$-$\grave{i}$, *i*-3, $\frac{8}{3}$-2, $\frac{5}{2}$-3, $\frac{5}{4}$-5 are rare; and *i*-4, 2-2, −6-$\frac{3}{2}$, −4-3, $\frac{3}{2}$-3, are very rare.

The following table of all the observed planes, with the lettering employed by different authors, is added for the sake of convenience of reference. Miller's letters are taken from Brooke and Miller's Mineralogy (1852); Schröder, from Pogg. Ann., xciv, 1855; Dauber, from Pogg., Ann., ciii, 1858. The planes in this list that have not been found by the author on the Bergen Hill crystals, have their symbols in parentheses.

Dana.	Mohs.	Miller.	Schrœder.	Dauber.	Des Cl.
O	*s*	*a*	*s*	*a*	h^1
ii	*b*	*c*	*b*	*c*	*p*
*i*4 (new)					
*i*2 (new)					
($i\frac{3}{2}$)					$e^{\frac{3}{2}}$
I	β Gregg				
$i\frac{3}{2}$	*d*	*d*	*d*	*d'*	e^1
$\grave{i}2$	*r*	*r*		*r'*	$e^{\frac{1}{2}}$
	o	*o*		*o'*	
$\grave{i}3$ (new)					$e^{\frac{1}{3}}$
ii	*u*	*b*	*o*	*b'*	*g'*
($-\frac{1}{2}i$)					$o^{\frac{1}{5}}$
$-1i$		*u*	γ	*u*	$o^{\frac{1}{4}}$
$-\frac{1}{3}i$		*v*			$o^{\frac{1}{3}}$
$-2i$	*a*	*x*	*a*	*x*	$o^{\frac{1}{2}}$
($-3i$)		*f*			$o^{\frac{2}{3}}$
$-4i$		ϕ			o'
$-6i$		*s*	*y*		$o^{\frac{3}{2}}$
$-8i$			*z*	ψ	o^2
($8i$)					a^2
($6i$)					$\frac{3}{2}$P∝Hess'b.
$2i$				γ''	$a^{\frac{1}{2}}$
$\frac{1}{2}i$ (new)					
$1i$		σ		σ	$h^{\frac{5}{3}}$
$\frac{4}{3}i$		*t*	*t*	*t*	h^2
$2i$	*g*	*g*	*g*	*g*	h^3
($\frac{8}{3}i$)					h^5
$4i$	*f*	*m*	*f*	*m*	*m*
($8i$)					g^3
−4	P	*n*	P	*n*	$d^{\frac{1}{2}}$
(−8)				ξ	δ
4	*n*			β'	$h^{\frac{1}{2}}$
2	*e*	*e*	*e*	*e'*	*e*
$\frac{4}{3}$	*l*		*l*	ϕ'	*d*
1	*m*	*l*		*l'*	μ
$\frac{4}{5}$		κ		κ'	κ
$\frac{2}{3}$ (new)					

Dana.	Mohs.	Miller.	Schrœder.	Danber.	Des Cl.
-42			q		
-44 (new)					
42 (new)					
$-6\frac{3}{2}$ (new)					
-63		ρ		ρ	q
$(-3\cdot3)$					
$(-3\frac{3}{2})$		ω			x
$(-5\frac{5}{3})$				χ	ζ
$(24\cdot3)$					z
$(-\widetilde{22})$					γ
$(-\frac{2}{3}\widetilde{2})$		i	α	i (h Gregg)	u
$-4\widetilde{2}$		z		z	$d\frac{1}{2}$
$-8\widetilde{2}$	q	q	β	q	β
$\frac{8}{3}\widetilde{2}$	i				
$2\widetilde{2}$	h	h	a	h	a
$-4\widetilde{3}$ (new)					
$\frac{3}{2}\widetilde{3}$ (new)					
$\frac{6}{5}\widetilde{3}$ (new)					
$(4\widetilde{4})$		y			$d\frac{1}{3}$
$-6\frac{3}{2}$(?) (new)					
$12\frac{3}{2}$	p	p			π
$\frac{6}{5}\widetilde{5}$ (new)					
$\frac{9}{4}\widetilde{9}$ (new)					
$\frac{8}{7}\widetilde{4}$(?) (new)					
$\frac{8}{9}\frac{4}{3}$(?) (new)					

In the figures the axes have the positions and the relative values adopted by Professor Dana, and the system of symbols employed is also the same. It is to be noticed that Lèvy adopted this position of the axes in his work on the Heuland Cabinet (1837), while other authors have taken i-i as O and made either 2-$\grave{\imath}$ or 4-$\grave{\imath}$ the fundamental prism. This position has the considerable advantage of giving the planes in vertical zones, and so making the form more symmetrical, and the mathematics more simple. It might be an improvement to double the length of the vertical axis; the theoretical form would then approach more closely to the dimensions commonly occurring in the crystals.

It is worthy of note that the planes of the fundamental *plus* octahedrons are represented by the terms in the series $\frac{4}{1}$ $\frac{4}{2}$ $\frac{4}{3}$ $\frac{4}{4}$ $\frac{4}{5}$ $\frac{4}{6}$ $\frac{4}{8}$; and excluding 8-$\grave{\imath}$ and $\frac{8}{3}$-$\grave{\imath}$, two clinodomes mentioned by Des Cloizeaux, the clinodomes are all of the same series, though wanting thus far the members $\frac{4}{5}$ and $\frac{4}{6}$.

In the preparation of this article I have had the free use of the specimens of datolite in the cabinets of Yale College, Prof. G. J. Brush, Rev. E. Seymour of New York, and Mr. Benjamin

Haines of New Jersey. The cabinet of Mr. Haines contains many hundred specimens, and I am greatly indebted to his kindness for the privilege of examining them at my leisure. To Mr. Seymour and Professor Brush I would also express my grateful acknowledgments. The complex form represented in figure 8 is from a crystal in the cabinet of Mr. Seymour.

New Haven, Ct., May, 1872.

ART. V.—*On certain Lower Silurian rocks in St. Lawrence county, N. Y., which are probably older than the Potsdam Sandstone;* by T. B. BROOKS.

A SURVEY of the Caledonia and Keene iron mines at Keene Station, St. Lawrence county, New York, made by me in the spring of 1870, developed the following series of sedimentary conformable rocks, some of which are apparently older than the Potsdam.

In descending order the series is as follows:—1st, A fine grained, somewhat friable light gray, sometimes reddish, sandstone, which toward the bottom of the bed is often a quartz conglomerate. It is lighter colored and less firm, but otherwise resembles the sandstone quarried at Potsdam. The maximum thickness observed was, say 40 feet, but the line separating this rock from No. 2 was not always well-defined, and the surface was lowered from erosion.

This rock is named by Dr. Emmons Potsdam sandstone, page 93, Part IV, Geology of N. Y., where the Caledonia Mine is described under the name of the "Parish ore bed."

2nd, Next below this sandstone is the iron ore formation; made up of red hematites, both specular and earthy, together with irregular lenticular masses of a brownish and very compact sandstone or quartzite, and a magnesian rock resembling No. 3 of this series. Associated with the ore are the carbonates of lime and iron and other minerals: carbonaceous matter is shown by the analyses. This formation varied greatly in thickness in different localities, from a few feet to at least 40. The mines which are now extensively worked are in this formation.

3rd, Under the ore, and forming the foot wall of the mines, is a soft rock, generally schistoze or slaty, but sometimes massive in structure, of a green to grayish-green color, weathering lead gray and becoming porous where exposed in outcrops. It is apparently a magnesian rock, containing considerable graphite and iron pyrites, designated by Dr. Emmons as serpentine, and

with the overlying ore was regarded by him as eruptive. This schist, like the ore, varies greatly in thickness, the maximum observed being at least 90 feet.

4th, Below the schist is a bed of granular, crystalline limestone, white to light gray in color; often friable near the surface and weathering to a dark color. It holds numerous crystals of bronze colored mica and, still more abundantly, graphite in thin scales. The thickness of this formation is not less than 250 feet.

5th, Is a sandstone similar in character to No. 1, described above. The thickness is uncertain, but one outcrop is exposed not less than 15 feet. Dr. Emmons does not mention this rock, and I do not think he observed it, or he certainly would have mentioned it in connection with his igneous theory for the origin of limestones, inasmuch as it separates two deposits of his "primitive limestone."

6th, Is a granular crystalline limestone closely resembling No. 4 before described, but differing in containing in places irregular beds or veins of granite, composed of a white feldspar and quartz. A mineral resembling tremolite was observed in this formation. This association of limestone and granite is fully described by Dr. Emmons (pages 24 and 338 and 340), and seems to have afforded him the best arguments for his peculiar views regarding the origin of the limestone. The thickness of this bed could not be even approximately determined; it is certainly thicker than the limestone already described.

As my survey was purely economic, having reference to explorations for iron ore, not much attention was paid to the thickness of the rocks below the ore. I consider that the series described has a minimum thickness of 700 feet, and probably much greater. Underlying this whole series and bounding it on the southeast side is a well characterized gneiss, which is in all probability non-conformable; but the actual contact was not seen. This gneiss is a part of the great azoic area of northern New York, and is colored Laurentian on the geological map of Canada. It is lithologically a totally different rock from the granite described above in No. 6 as associated with the marble, although Dr. Emmons seems to give them the same origin and age. No limestone was seen in it,—the feldspar was reddish, and it always contained mica and often hornblende.

This series of sandstones, limestones, and ferruginous and magnesian schists was not found complete, so as to display unmistakably the sequence of the beds except on one section, i. e., through the west corner of the Caledonia mine lot:—and there the 2nd and 3rd members were of less than usual thickness. The Laurentian gneiss was not seen on this section. At the Kearny mine the members of the series from 1 to 4 inclusive

are well shown. At the Caledonia mine the 1st, 2nd and 3rd are well developed, as is also the case southeast of the Keene. At this latter point the magnesian schist No. 3 is seen in such close proximity to the gneiss as to render it probable that beds 4 to 6 are wanting. This could be explained by supposing great irregularity in the bottom of the sea in which they were deposited.

The whole series are folded, presenting several anticlinal and synclinal axes which run rudely parallel with the edge of the Laurentian area, i. e., northeast and southwest. On one section a half-mile long across the Caledonia and Kearney mines, no less than six such axes were observed. Prof. Dana describes the rocks of the Potsdam epoch in New York as having usually "a gentle dip or as nearly horizontal." This description would not embrace the rocks at this locality. In one place an outcrop of magnesian schist (No. 3) has a dip of 80°, indicating a very sharp fold. Aside from this, the inclinations observed varied from 0° to 40°. The upper sandstone has been eroded from considerable part of the area about the mines, exposing the lower rocks and affording a good opportunity for stratigraphical study. The surface is quite hilly, the highest point observed being 120 feet above the valley at its base.

As has been remarked, Dr. Emmons describes formations Nos. 1, 2, and 3 in Geology of New York, Part IV, page 93, under the respective names of "Potsdam Sandstone," "Specular Iron Ore" and "Serpentine." He regards the last two as well as the limestone which underlies them as eruptive, and does not seem to have observed the sandstone which divides the two limestone beds. The planes of bedding were occasionally obscure in the magnesian rock and sandstones, often so in the iron ore and marbles; but taken as a whole they cannot be regarded as other than sedimentary rocks showing more or less evidence of metamorphic action. Except as noted above, I do not find them described by the New York geologists.

Regarding their age, if Dr. Emmons is right in calling the uppermost sandstone No. 1, "Potsdam," as he has repeatedly done, and if he begins the Potsdam epoch at this locality with the bottom of this sandstone, which is unquestionably his intention, then the rock beds 2 to 6 inclusive are older than this so-called base of the Paleozoic column. This view would possibly make them the equivalents of the Taconic system of Dr. Emmons, and it is strengthened by the similarity in general lithological character. number and order of the beds between this series and that system as described in Geology of New York, Part IV, pages 138 to 144. It is hardly to be supposed, however, that Dr. Emmons would have passed over so prominent a suggestion of his favorite system without recognizing it.

There is no doubt but that his peculiar views regarding the origin of limestones stood somewhat in the way of his seeing all the facts that this interesting locality exhibits. His section of the Parish ore bed, page 93, Part IV, does not represent the facts now to be observed. I found only magnesian schist where he has marked gneiss. It should be remarked, however, that there has been a large amount of work done of late years, revealing many additional facts.

Dr. Emmons gives 60 to 300 feet as the minimum and maximum thicknesses of the Potsdam sandstone in northern New York, and it has been described as diminishing in some instances to 20 or 30 feet. The prevailing rock observed in this region seems to have been a laminated sandstone, frequently having a conglomerate as its lower member. Nor have any rocks of different lithological character been ascribed to this period in the region in question; although partly calcareous layers and even beds of true limestone have been observed in the upper rocks of this period in the northwest. The rocks I have described, therefore, have apparently too great a thickness and show too wide a variation in lithological character to be regarded as the equivalents of the Potsdam. Some forms, obtained from a calcareous layer the base of the upper sandstone, which I thought might be organic, were pronounced by Dr. Newberry to be concretions. The abundance of graphite was the only evidence of organic life observed.

The Potsdam quarries are only 35 miles northeasterly from the Rossie mines, and the country between was examined by Dr. Emmons. He may have traced the sandstone through stratigraphically. My own reconnaissance of this country leads me to believe that this could easily be accomplished. Should it be done, a point of considerable interest which would be incidentally settled is this:—Does the Potsdam sandstone in this region become, in places, decidedly gneissic in character? My own hasty observations at the Sterling and Tate ore beds leads me to believe that it does: if so, the Tate bed described by Dr. Emmons, pages 95 and 346, as being overlaid by gneiss, may be found to be the equivalent of the Rossie beds, which I believe to be the case. The ores are certainly very similar. This Tate bed confirmed Dr. Emmons, and with good reason, in his view, that the iron ores were of "primitive age," lying either in or immediately on top of the great gneissic series.

As bearing on this subject, I would mention that the iron ores of the Maramec district in Crawford and Phelps Co., Mo., bear a close resemblance to those of Rossie, and work equally well in the furnace. The Missouri ore contains considerable yellow ochre, which is less abundant at Rossie; but

the specular and earthy red hematites are nearly identical and unlike any other iron ore I have seen. A sandstone associated with the Missouri ores is very similar to the Potsdam in lithological character, and the series is, I believe, regarded by the Missouri geologists as of Lower Silurian age. Iron ores are described as occurring in the Potsdam period of Canada, but I do not know of any ore in the United States which the Geological Reports assign to that period.

ART. VI.—*On a simple Apparatus for the Production of Ozone with Electricity of high tension;* by Prof. ARTHUR W. WRIGHT.

EXPERIMENT has shown that in the production of ozone by electricity the maximum amount of oxygen is ozonized by the silent or glow discharge, and most of the forms of apparatus by which this is effected are contrivances by which oxygen is made to flow slowly through a space traversed by such a discharge. In v. Babo's apparatus, as well as in those of Siemens and Houzeau, the metallic conductors are separated by glass and a stratum of air. By inductive action of the charged metallic surfaces the intervening air becomes charged with electricity oppositely upon its two sides, and simultaneously with the discharge of the metallic terminals, through the wire of the coil, a discharge takes place through the air, not in the form of sparks, but diffusely, producing a glow of purplish light, visible only in the dark.

These apparatus succeed best with electricity of comparatively low tension. In using the Holtz's electro-machine with them the discharge is apt to occur chiefly in the form of sparks through the air, or it may even traverse and perforate the glass, and the form of the apparatus must be varied to give the best results.

When the poles of the machine itself are separated to a sufficient distance the electricity passes between them either in the form of a diffuse brush, spanning the whole interval, or with a very minute brush upon the negative pole, and a glow upon the positive, the intermediate space not being visibly luminous. This is the so-called dark or silent discharge, exhibiting the phenomena of the electric shadow when suitable objects are interposed, as described in a former paper.* When this occurs the strong odor shows that a considerable amount of the atmospheric oxygen is converted into ozone.

* This Journal, II, xlix, p. 381, and III, i, p. 437.

If this discharge is made to take place in an enclosed space through which air or oxygen can be driven, the ozonizing effect of the electricity is heightened and can be utilized. The apparatus which I have employed, and which has afforded very satisfactory results, consists of a straight glass tube about 20 centimeters long and having an internal diameter of 2·5 centimeters, the two ends being stopped with corks covered on the inner side with a thin coating of cement to protect them from the action of the ozone. Through the axis of each cork is inserted a glass tube of about 5 millimeters caliber, and 7 centimeters in length, having a branch tube inserted perpendicularly at the middle and long enough to permit a rubber tube to be slipped upon it. The outer ends of the tubes themselves are closely stopped with corks, through which are passed straight, thick copper wires carrying suitable terminals at their inner ends, and bent into a ring at the others. They are fitted so as to make tight joints, but to allow of motion in order to vary the distance between their inner ends. One of these wires carries a small ball; the other terminates in a disk with rounded edge, set perpendicularly to the axis of the tube, and so large as to leave an annular space of some two or three millimeters breadth around it. The gas is admitted through one of the branch tubes and escapes from the other, after having passed through the whole length of the tube.

In using the apparatus the wires must be connected with the poles of the machine in such a manner that the disk becomes the negative terminal, as this arrangement gives the greatest degree of expansion and diffuseness to the current. On turning the machine, and adjusting the ball and disk to a proper distance, a nebulous aigrette surrounds the latter, quite filling the interval between it and the wall of the tube, while the part of the tube between the disk and ball is crowded with innumerable hazy streams converging upon the positive pole, or simply causing the latter to be covered with a faint glow. A current of air or oxygen sent into the tube must pass through this, and ozone is very rapidly produced, and in great quantity. The condensers are of course not used with the machine, when this apparatus is employed.

There appears to be an advantage in causing the oxygen to pass from the negative toward the positive within the tube, for the gas through which the discharge passes is transported in the contrary direction, as may be readily seen on bringing a candle flame between the poles of the machine, or causing a thin column of smoke to rise through the polar interval. The flame and the smoke are deflected, and stream off toward the negative pole. If the gas should be admitted in the direction mentioned, there would be a tendency to obstruct its flow some-

what, and thus keep it longer under the influence of the electricity.

Some experiments which were made with the apparatus will give an idea of its efficiency. One hundred cubic centimeters of water were placed in an upright tube or test-glass, and into it were put 20 drops of strong indigo solution, causing it to assume a deep blue tint. Air was driven through the ozonizing tube, under a pressure of about three inches of water, and on issuing from it conveyed by a tube into the solution. When the electro-machine was put in operation, being turned with sufficient speed to give nearly its maximum effect, the solution completely lost its blue color in less than four minutes. Blue litmus solution under similar circumstances became pale pink, but required a considerably longer time for the change.

When Schönbein's test solution is employed the deep blue color is immediately produced, but the solution is too thick to work well if the starch has been heated considerably, or for a long time, in making it. A better proportion is to take one part of potassic iodide by weight, ten parts of starch, and five thousand parts of water. This forms a milky solution, sufficiently mobile to mix well when the ozonized air bubbles through it. When 100 cubic centimeters of this solution were used, and air passed through the apparatus as before, the blue color appeared at once on application of electricity, and in 30 seconds it was deeply colored.

With dry oxygen the effects were much more rapid and remarkable. 100 cubic centimeters of the solution were used, as before. The instant the machine was put in action the liquid about the end of the delivery tube became deep blue, and in from ten to fifteen seconds the whole had acquired a uniform and intense blue color.

The summer moisture having interfered somewhat with the effective working of the electro-machine, there has been no opportunity to determine the percentage of ozone produced in this manner, but it appears to be very large. When dry oxygen is passed through the tube very slowly, the issuing gas when inhaled produces a painful burning sensation in the lungs, and causes violent coughing, which persists for a considerable time.

When oxygen is used it is found that the electrodes must be separated to a much greater distance than is necessary for air, otherwise sparks pass and destroy a large proportion of the ozone already formed. With air the direct spark in the apparatus could not be made to pass over an interval of more than 7 centimeters, but in oxygen they did not cease until the poles were separated about 11·5 centimeters. When the tube was filled with air and the poles were 7 or 8 centimeters apart the

discharge was of the silent kind, but on admitting oxygen it immediately took the form of direct sparks.

The quantity of the solution used in these experiments was much greater than would be needed in order to exhibit the characteristic reactions of ozone to an audience of moderate size. One-half or one-third of the amount would be quite sufficient, and the time required for the reaction would be proportionally shorter. The great quantity of the ozone, as well as the ease and rapidity with which it is produced, render the apparatus especially serviceable for use in the lecture-room.

ART. VII.—*On the Action of Ozone upon Vulcanized Caoutchouc;* by Prof. ARTHUR W. WRIGHT.

IN using the Holtz's electro-machine, in the summer season, it is often very difficult to make it retain any considerable charge, or even to keep up its action for more than a few minutes. The ebonite insulators are found to have lost in a large degree their insulating power, and to have become conductors to such an extent that considerable sparks may be drawn from them at points several inches distant from the metal parts supported by them, thus dissipating the greater portion of the charge. This is the usual condition of things when the machine, after much use, has stood for some weeks in the warmer portion of the year unused. The surface of the ebonite becomes hygroscopic, condensing upon itself a large amount of moisture, the accumulated liquid being sometimes so abundant as to trickle down in drops.

Having noticed on one occasion that this liquid had an acid taste, I was led to examine it more closely, and the ordinary tests very speedily showed it to be sulphuric acid. Its presence was a sufficient explanation of the defective insulation. Similar deposits of moisture were found upon the ebonite jackets of two induction coils some time after they had been used.

As nothing containing sulphur had been used about the apparatus, the acid was evidently derived from the ebonite itself. The first thought was that the material had been heated in the process of vulcanization sufficiently to oxidize the sulphur; but as the sulphurous oxide, if thus formed, would be dissipated by the heat, this could hardly be regarded as the source of the sulphuric acid, especially as the latter did not appear until after the apparatus had been used. It is well known that vulcanized caoutchouc is affected by ozone, and that the ordinary rubber tubes through which it is passed are

attacked and quickly perforated by it. It seemed most probable then that the acid was the result of the action of the ozone upon the insulators, and experiments were made which entirely confirmed this supposition.

To the exit-tube of the ozonizing apparatus described in the previous paper was attached one end of a vulcanized rubber tube a few inches long, the other end being slipped upon the glass tube of a small wash-bottle containing some thirty or forty cubic centimeters of water. Air was slowly driven through the apparatus, and, having been strongly ozonized by the action of the electricity, bubbled up through the water. This was continued for an hour and a half. At the end of this time common air was passed through the apparatus to displace the ozone left in it, the tubes were removed, and the bottle closed with a glass stopper. On opening the bottle some time afterward, there was an unmistakable odor of sulphurous oxide, and the water reddened blue litmus paper very quickly and strongly. A strip of litmus paper, hung in the bottle so as not to touch the water, was completely reddened in a short time, and this happened even after several days had elapsed from the time of the experiment. The water tested with chloride of barium gave a considerable crystalline precipitate, leaving no doubt of the presence of sulphuric acid.

A small slip was cut from a thin plate of ebonite, cleaned and dried, and placed in a small bottle into which ozone was driven as before. In a short time it was bedewed with moisture having an acid taste, and exhibiting the same properties as that found upon the insulators of the machine.

In order to determine whether the sulphur itself could be directly oxidized by ozone, a quantity of fine flowers of sulphur was gently rubbed into a loose lock of dry cotton, so as to diffuse it as much as possible. The cotton was placed in a dry wash-bottle, and connected by means of a glass tube with a second wash-bottle containing a little water, all the connecting tubes being of glass. Ozone was passed through the bottles for an hour and a half, but at the end of this time not the slightest evidence of any action upon the sulphur could be detected. This was what might have been expected, for as the air often contains a small percentage of ozone, sulphur exposed to it would undergo slow alteration, with loss of weight, and it does not appear that anything of the kind has ever been observed.

It is evident that while the ebonite is undergoing decomposition by the ozone, the oxygen combines with the issuing sulphur to form sulphurous oxide, which with the atmospheric moisture produces sulphurous acid, this in turn being converted into sulphuric acid by the further action of the ozone. The

absorption of moisture from the atmosphere by the sulphuric acid produces the dew-like deposits observed.

The deleterious effect upon the insulators can be remedied by neutralizing the acid with some substance which will not form a hygroscopic compound or essentially lessen the insulating power of the ebonite. I have used oxide and carbonate of magnesium with very good effect. A little of either of these substances in fine powder is sprinkled upon a soft cloth or piece of chamois leather and rubbed over the insulators. The excess is removed with a wet cloth, and the surface, after drying, cleaned and polished by rubbing with a soft woolen cloth very slightly moistened with carbonic di-sulphide. As the ebonite is attacked by the latter substance, care should be observed, in employing it, to use only so much as is needed to facilitate the polishing process without injuring the surface. The ebonite may be somewhat discolored by these operations, but the color can be restored by rubbing with a little oil, or will return of itself after a time.

Probably a better method may be found, but this gives very good results. On one occasion, early last autumn, when the electro-machine had not been used for some months, the sparks obtained on charging it and using small condensers were only about one quarter of an inch in length, and the action of the apparatus was very feeble. The insulators were quite damp with the accumulated moisture. When this had been removed by the process described, sparks eight or nine inches in length were obtained at once, and the machine worked with nearly its usual energy.

Art. VIII.—*On the Oceanic Coral Island Subsidence;* by James D. Dana.*

Coral islands have been shown to be literally monuments erected over departed lands; and, through the evidence from such records, it is discovered that the Pacific has its deep-water mountain chains, or lines of volcanic summits, not merely hundreds, but thousands of miles in length. Some of the ranges of high islands are proved by such records to have an under-water prolongation, longer than that above water: the line of the Hawaiian Islands, for example, which has a length of only four hundred miles from Hawaii to Kauai, and five hundred and thirty to Bird Island, the western rocky islet of the group, but stretches on westward, as the coral registers show,

* From the closing chapter of the writer's work on Corals and Coral Islands, (pp. 364–372), recently published by Dodd & Mead, New York.

even to a distance of two thousand miles from Hawaii, or as far as from New York to Salt Lake City; and how much farther is unknown, as the line of coral islands here passes the boundary of the coral reef seas, or the region where coral records are possible.

Other ranges of submerged summits are shown to extend through the whole central Pacific, even where not a rocky peak remains above the surface; for all the coral islands from the eastern Paumotus to Wakes Island, near long. 170° E. and lat. 19° N., north of the Ralick and Radack (or Marshall) groups, are in linear ranges: and they have, along with the equally linear ranges of high islands just south, a nearly uniform trend, curving into northwest and north-northwest at the western extremity. The coral islands consequently cap the summits of linear ranges of elevations, and all these linear ranges together constitute a grand chain of heights, the whole over five thousand miles in length. Thus, the coral islands are records of the earth's submarine orography, as well as of slow changes of level in the ocean's bottom.

This coral island subsidence is an example of one of the great secular movements of the earth's crust. The axis of the subsiding area* has a length of more than six thousand miles —equal to one-quarter of the circumference of the globe; and the breadth, reckoning only from the Sandwich Islands to the Friendly Group (or to Tongatabu) is over twenty-five hundred miles, thus equalling the width of the North American continent. A movement of such extent, involving so large a part of the earth's crust, could not have been a local change of level, but one in which the whole sphere was concerned as a unit; for all parts, whether participating or not, must have in some way been in sympathy with it.

This subsidence was in progress, in all probability, during the Glacial era, the thickness of the reefs proving that in their origin they run back through a very long age, if not also into the Tertiary. It was a downward movement for the tropical Pacific, and perhaps for the warmer latitudes of all the oceanic areas, while the more northern continental lands, or at least those of North America, were making their *upward* movement, preparatory to or during that era of ice.

The subsidence connected with the origin of coral islands and barrier reefs in the Pacific has been shown† to have amounted to several thousands of feet, perhaps full ten thousand. And, it may be here repeated that, although this sounds large, the change of level is not greater than the *elevation* which

* The position of this area is stated on page 328 of the volume on Corals and Coral Islands.

† Ibid, p. 329.

the Rocky Mountains, Andes, Alps and Himalayas have each experienced since the close of the Cretaceous era, or the early Tertiary; and perhaps it does not exceed the upward bulging in the Glacial era of part of northern North America.

The author has presented reasons for believing* that in this Glacial era the watershed of Canada, between the River St. Lawrence and Hudson's Bay, was raised at least 5,500 feet above its present level (1,500 feet); and that this plateau thus elevated was the origin of the great glacier which moved southeastward over New England. This region is the summit of the eastern arm of the great V-shaped Azoic area of the continent, the earliest elevated land of North America; and it is not improbable that the other arm of the V, reaching from Lake Superior and Huron, northwestward, to the Arctic, was raised at the same time to a higher elevation, and was the source of glacial movements over the more central portions of the continent:—we cannot say *western* portions also, since, in the *first* place, the facts, according to Prof. J. D. Whitney, do not sustain the statement; and, in the *second*, the great mountain ranges of the west would have been a barrier to all influences from any central continental elevation, and, besides, the slopes of these ranges, even if the Pacific border were higher to the north than now, would have in general determined the course of the western glacial movements.

The idea that the two arms of the great Azoic V were raised together, is not without some support. For the courses of the two were the courses of great continental uplifts or movements, again and again, through the successive subsequent ages; and the present outline of the continent is but the final expression of the great fact; moreover, the elevations parallel to the western arm of the V have been much the greatest. Even the exceptional courses, such as the nearly north and south trend of the Green Mountains, were marked out first in the Azoic, the Azoic peninsula of northern New York with the line of the Adirondacks being an exhibition of it. And all this uniformity of movement, from the laying of the first stone in the developing continent to the last, has been shown by the author to be directly connected with the fact that the continent has always been bordered by the same two great oceanic depressions, the Atlantic and the larger Pacific, the same in trend of axis as now, the North Atlantic having a northeast and southwest trend, parallel with one arm of the Azoic, and the Pacific a northwest and southeast parallel with the other arm of the Azoic. It is therefore reasonable that, late in geological history, during the Glacial era, after the great mountain chains of

* This Journal, III, ii, 1871.

the continent had been made and raised to their full height, and the surface crust thickened over all the continent, except that of the Azoic nucleus, by successive beds to a thickness of thousands of feet, even thirty-five thousand by the close of the Paleozoic along the Appalachians, and probably much beyond this on the Pacific border; and when these thick sediments had in many regions been stiffened by crystallization or metamorphism; I say it is reasonable that, finally, changes of level, through the working of the old system of forces, should again have affected most the old nucleal Azoic area of the continent, where there had been no thickening except what had taken place internally; and that, if one arm of the V, that along the Canadian watershed, were raised at this time—as the facts prove—the other, northwestern in trend, should also have been raised, and to a greater extent. This is at least probable enough to become a question for special examination over the region.

These northern continental upward movements which introduced the Glacial era, carrying the Arctic far toward the tropics, may have been a balance to the *downward* oceanic movements that resulted in the formation of the Pacific atolls. While the crust was arching upward over the former (not rising into mountains, but simply arching upward), it may have been bending downward over the vast central area of the great ocean.

The changes which took place, cotemporaneously, in the Atlantic tropics, are very imperfectly recorded. The Bahamas show by their form and position that they cover a submerged land of large area stretching over six hundred miles from northwest to southeast. The long line of reefs and the Florida Keys, trending far away from the land of southern Florida, are evidence that this Florida region participated in the downward movement, though to a less extent than the Bahamas. Again, the islands of the West Indies diminish in size to the eastward, being quite small in the long line that looks out upon the blank ocean, just as if the subsidence increased in that direction. Finally, the Atlantic beyond is water only, as if it had been made a blank by the sinking of its lands.

Thus the size of the islands, as well as the existence of coral banks, and also the blankness of the ocean's surface, all appear to bear evidence to a great subsidence.

The peninsula of Florida, Cuba, and the Bahamas look, as they lie together, as if all were once part of a greater Florida or southeastern prolongation of the continent. The northwestern and southwestern trends, characterizing the great features of the American continent, run through the whole like a warp and woof structure, binding them together in one system; the former trend, the northwest, existing in Florida and the Bahamas, and

the main line of Cuba; and the latter course, the west-southwest, in cross lines of islands in the Bahamas (one at the north extremity, another in the line of Nassau, and others to the southeast), in the high lands of northwestern and southeastern Cuba, and in the Florida line of reefs, and even further, in a submerged ridge between Florida and Cuba. This combination of the two continental trends shows that the lands are one in system, if they were never one in continuous dry land.

We cannot here infer that there was a *regular* increase of subsidence from Florida eastward, or that Florida and Cuba participated in it equally with the intermediate or adjoining seas; for the facts in the Pacific have shown that the subsiding oceanic area had its nearly parallel bands of greater and less subsidence, that areas of greatest sinking alternated with others of less, as explained on page 328; and that the groups of high islands are along the bands of least sinking. So in the Atlantic, the subsidence was probably much greater between Florida and Cuba than in the peninsula of Florida itself; and greater along the Caribbean Sea parallel with Cuba, as well as along the Bahama reefs, than in Cuba.

The position of the lonely Bermuda atoll confirms these deductions. Its solitary state is reason for suspecting that great changes have taken place about it; for it is not natural for islands to be alone. The tongue of warm water due to the Gulf Stream, in which the Bermudas lie, is narrow, and an island a hundred miles or more distant to the northeast-by-east, or in the line of its trend, if experiencing the same subsidence that made the Bermuda land an atoll, would have disappeared without a coral monument to bear record to its former existence. Twenty miles to the southwest-by-west from the Bermudas, there are two submerged banks, twenty to forty-seven fathoms under water, showing that the Bermudas are not completely alone, and demonstrating that they cover a summit in a range of heights; and it may have been a long range.

In the Indian ocean, again, there is evidence that the coral-island subsidence was one that affected the oceanic area more than the adjoining borders of the continent, and most, the central parts of the ocean. For, in the first place, the Archipelago of the Maldives narrows and deepens to the southward. Further, the large Chagos Group, lying to the south of the Maldives, as remarked upon by Darwin, contains but very little dry land in any of its extensive reefs, while some of them, including the Great Chagos Bank, are sunken atolls. Again, still other large reefs nearly bare, lie to the southwest of the Chagos Group; while Keeling's is another outlying atoll southwest of southern Sumatra and far out toward mid-ocean.

The probability is, therefore, that both the central Atlantic and Indian Oceans were regions of this subsidence, like the central Pacific, and that the absence of islands over a large part of their interiors may be a consequence of it. A rate of sinking exceeding five feet in a thousand years (if my estimate from the growth of corals is right) would have buried islands and reefs together in the ocean; while, with a slower rate, the reefs might have kept themselves at the water's surface. So small may have been the difference of rate in the great movement that covered the Pacific with coral islands, but left the Indian Ocean a region of comparatively barren waters, with some "half-drowned" atolls, and the central Atlantic almost wholly a blank.

While thus seeming to prove that all the great oceans have their buried lands, we are far from establishing that these lands were oceanic continents. For as the author has elsewhere shown, the profoundest facts in the earth's history prove that the oceans have always been oceans. These lands in all probability were, for the most part, volcanic islands or summits of volcanic ranges, for of this nature are all the islands over the interior of either ocean that are not of coral origin.

The course of argument leads us to the belief that a very large number of islands, more than has been supposed, lie buried in the ocean. Coral islands give us the location of many of these lands; but still we know little of the extent to which the earth's ranges of heights, or at least of volcanic peaks, have disappeared through oceanic subsidence. Recent dredgings and soundings have proved that the bottom of the oceanic basin has little of the diversity of mountain chains and valleys that prevail over the continents; and, through this observation (and also by the discovery that some ancient types of animal life, supposed to have been long extinct, are perpetuated there), they have afforded new demonstration of the proposition, above stated, that *the oceans have always been oceans.* But while the facts do not imply the existence deep in the ocean of many granitic mountain chains, they do teach that there are long ranges, or lines, of volcanic ridges and peaks, and some of these may be among the discoveries of future dredging expeditions. A range of deep-sea cones, or sunken volcanic islands, would be as interesting a discovery as a deep-sea sponge or coral, even if it should refuse, excepting perhaps a mere fragment, to come to the surface in the dredge.

We may also accept, with some confidence, the conclusion that atolls and barrier reefs originated in the same great balance-like movement of the earth's crust that gave elevation and cold, in the Glacial era, to high-latitude lands. If so, the tropics and the colder latitudes were performing their several

works simultaneously in preparation for the coming era; and it is a gain to us in our contemplations, that we hence may balance the beauty and repose of the tropics, through all the progressing changes, against the prolonged scenes of glacial desolation that prevailed over large portions of the continents.

ART. IX.—*On a precise Method of tracing the Progress and of determining the Boundary of a Wave of Conducted Heat;* by ALFRED M. MAYER, Ph.D., Professor of Physics in the Stevens Institute of Technology, Hoboken, N. J.

IN 1870 Meusel experimented on the formation of double iodides, and on the remarkable changes of color produced in these bodies by heat.* He prepared a double iodide of copper and mercury, by adding to a warm solution of mercuric iodide in potassium iodide, copper sulphate and then sulphurous acid; the resulting precipitate is of a brilliant carmine red and (in that experimented on by me) turns to a deep chocolate brown on heating to about 70° C. In order forcibly to exhibit this change of color, Boettger moistened the iodide with weak gum water, and painted it on paper; on heating the latter, the change of color is produced, and on cooling, the iodide regains its former brilliancy.

Dr. G. F. Barker had the kindness to present me with a card so prepared, and on experimenting with it I soon perceived the valuable means it afforded of tracing the progress and of determining the boundary of a wave of conducted heat. To Dr. Barker I am also indebted for the iodide used in the experiments I here present.

The first use I made of this substance was to track the heat conducted by bars and plates of metal,† and the sharpness of the boundary of the colors instigated me to test the value of this mode of experiment, by applying it to a determination of the elliptical contour of the isothermal of conduction, in the principal section of a quartz crystal.

Sénarmont, in his beautiful researches on this subject (Ann. de Ch. et de Ph., 3e S., t. xxi, xxii), has carefully determined the ratio of the axes of this elliptical figure, by coating a thin longitudinal section of the crystal with wax, and leading through it a silver wire, by means of which heat was brought

* Ber. Berl. Chem. Ges., iii, 123, 1870. Bul. Soc. Ch., II, xiii, 220, 1870. J. Pr. Ch., II, ii, 136, Aug., 1870

† The iodide is decomposed by contact with certain metals; these should be coated with a film of collodion, or electrotyped with copper before applying the iodide.

to the center of the plate, whence it was conducted outward, and its progress and isothermal contour determined by the melting of the wax. The following are Sénarmont's experiments on a plate 27^{mm} square, whose sides were parallel and perpendicular to the principal axis of the crystal:

Exp.	Major Axis.	Minor Axis.	Ratios.
1	12·50	9·75	1·28
2	11·60	8·50	1·35
3	10·00	7·50	1·33
4	12·00	9·00	1·33
5	13·75	10·00	1·37
6	18·00	14·00	1·29
7	15·00	12·00	1·25
8	9·75	7·50	1·30
			1·31 Mean Ratio.

Sénarmont, in the above experiments, used every precaution to attain accurate results. He screened the plate from draughts of air and from radiations; kept the plate horizontal and frequently rotated it around its heated wire. After the ellipse had become constant in its form, he allowed the plate to cool, and then measured the axes of the ellipse by means of a micrometer.

In the experiments which follow, I used a quartz plate 27^{mm} long, 22^{mm} wide, and whose thickness was $1{\cdot}2^{mm}$. Its center of figure was pierced by a hole $1{\cdot}25^{mm}$ in diameter, through which passed the vertical conical end of a silver wire. The iodide was made into a paint with weak gum water, and in experiments 1, 2, 3 and 4 was applied to the surface of the plate by a camel's hair pencil. In experiments 5, 6, 7 and 8, the better plan was adopted of flowing the iodide over the plate, and allowing the water spontaneously to evaporate. Thus we obtain a smooth, evenly distributed coating, giving a sharp outline to the elliptical figure of the conducted heat. The plate was screened from radiations of the flame which heated the wire, but was not shielded from currents of air, nor was unequal radiation of the iodide specially prevented. The method of measurement was as follows: after the ellipse was well formed, and of permanent dimension, the extremities of its longer and of its shorter axes were marked by scratching through the iodide with a very slender steel point; the plate was then removed, and the lengths of the axes determined by means of dividers and a scale divided into half millimeters.

Exp.	Major Axis.	Minor Axis.	Ratios.
1	12·5	9·25	1·35
2	14·0	10·5	1·33
3	17·75	13·5	1·31
4	18·25	14·0	1·30
5	12·75	9·5	1·34
6	12·8	9·5	1·34
7	12·8	9·5	1·34
8	16·4	11·8	1·38
			1·33 Mean Ratio.

An opinion on the relative values of the two modes of experimenting can only be formed from a discussion of the two series of observations by the method of least squares. It is true that the series are not as extended as one would wish for the application of this process, yet its results are equally fair for both. We thus have found that the—

Probable error of a single determination of ratios in S.'s series is ·0267
" " " " M.'s " ·0170
" in the mean ratio " S.'s " ·0094
" " " M.'s " ·0060

From these figures we infer that Sénarmont's ratio is barely true to a hundredth, while my result can be relied on to that figure, and if my measures had been made with a micrometer microscope, on a plate protected from unequal radiation, and shielded from currents of air, I would have obtained a ratio reliable to the third decimal place.

To the higher ratio of my determination I attach no importance; I attribute it to the peculiarity of this particular crystal, for several measures on this plate, with a waxed surface, gave even a higher ratio than when the iodide was used. It hence appears that to obtain the correct ratio for a given crystal, the mean ratio derived from several plates should be adopted.

The remarkable change of color which heat produces in this iodide led me to hope that this molecular change would be accompanied by a simultaneous variation in its radiating power. To solve this problem, I made the following experiments, at different temperatures, below and above the degree at which its color changes. One side of a hot-water cube was coated with lamp-black, and another side with a thick paste of iodide and gum-water; after the latter had nearly dried, I sifted iodide over it, and caused this to adhere by rubbing it gently with my finger. The cube was now filled with water, in which was supported a thermometer. The water was raised to the following temperatures, and frequently agitated, so as to ensure a uniform heating of the cube. The deflections produced in the galvanometer needles by the lamp-black, and by the iodide,

were then obtained for each fixed temperature. Each deflection given below is the mean of three experiments.

Temp.	Lamp-black.	Iodide.	Ratio of Deflections.	Changes in Color.
60°	18·75	13·75	1 : ·70	
65°	22·25	17·	1 : ·71	Cherry red, and turning in spots to chocolate color.
68°	22·75	16·25	1 : ·71	Dark red, with spots of chocolate color.
70°	24·0	16·87	1 : ·70	Whole surface of a deep brown.
72°	25·0	17·62	1 : ·70	Deep purplish brown.
75°	26·25	18·62	1 : ·70	" "
100°	45·0	30·5	1 : ·67	" "

The last experiment, in which the temperature of the surface was 100°, gave deflections so far exceeding those produced before that I sought to render them comparable by removing the hot water cube to a greater distance from the thermo-battery, when I obtained the following ratio:

Temp.	Lamp-black.	Iodide.	Ratio.
100°	20°	13·41°	1 : ·67

The result was the same ratio as formerly obtained.

These experiments seem to show that the molecular change in the iodide, which causes it to act so differently in reflecting light, does not appear to have any action on its power of radiating the rays of heat of low intensity. I intend, however, to return to this investigation, provided with an apparatus giving the differential actions of two cubes, and having a carefully calibrated galvanometer, and with this arrangement to test the reflecting as well as the radiating power of this and other iodides.

Several applications of this iodide for showing elevations of temperature will naturally present themselves; for example, Foucault's experiment of the heating of a copper disc, when rotating in the magnetic field, can be exhibited to a large audience by painting the disc with this iodide; on the disc attaining 70° C., the brilliant scarlet will change to a deep brown, to regain its former brilliant hue on cooling.

A more useful application may be made of this, or of several other more appropriate metallic compounds, by painting them on the *pillow-blocks*, and other parts of machines which are liable to injurious heating from friction. Thus the machinist can, from the colors of these paints, ascertain the temperature of these sometimes inaccessible parts of moving machines.

May 20, 1872.

ART. X.—*Remarks on the late Criticisms of Prof. Dana;* by T. STERRY HUNT, LL.D., F.R.S.

IN this Journal for February last (p. 86) Prof. Dana has criticized certain points in my address "On the Geognosy of the Appalachians and the Origin of Crystalline Rocks," given in August, 1871, at Indianapolis, before the American Association for the Advancement of Science. I am charged by him with rejecting, for many mineral silicates, the view that they are pseudomorphs; that is to say, crystals chemically altered without loss of external form. I have denied that crystals of serpentine having the shape of chrysolite, pyroxene, dolomite, etc., and crystals of pinite having the shapes of nepheline or scapolite, are results of a chemical change of these species; notwithstanding this view is now held by most mineralogists on the grounds of similarities of geometrical form and the existence of what are regarded as intermediate stages in the process of transmutation; and I have maintained another and a very different view, which, in my opinion, is more rational. Until we can watch the transmutation of one of these species into another, the argument from supposed intermediate forms is worth no more in the mineral than in the organic world; the reasoning of the transmutationists, in the one case and the other, resting upon somewhat similar considerations. In either case we may say, with Prof. Warrington Smyth, that in these intermediate forms "lie the materials for a history;" while we venture, with him, to express a doubt whether, from a series of specimens supposed to show a transition from chrysolite to serpentine, or from hornblende to chlorite, "we are obliged to conclude that there has been, *historically speaking*, an actual transition from the one to the other." [See his anniversary address, as president of the Geological Society of London, in 1867.]

Prof. Dana says that Scheerer is the only one who shares my peculiar views on this question. I have, however, asserted in my address that Delesse has maintained the views of Scheerer and myself, as opposed to the popular doctrine of epigenesis, and shall endeavor to make good my assertion. In his essay on Pseudomorphs, published in 1859 [Ann. des Mines, V, xvi, 317–392], Delesse begins his argument by remarking that since, in some cases, a mineral is found to be surrounded by another clearly resulting from its alteration (as for example anhydrite by gypsum), certain mineralogists have supposed that wherever one mineral encloses another there has been epigenesis or pseudomorphous alteration. Such, he says, may sometimes be the case, but it is easy to see that it is not so habitually. A crys-

tallized mineral species frequently includes a large and even a predominating portion of another, and the combination is then considered by many as an example of partial pseudomorphous alteration. In such instances, remarks Delesse, the question arises whether we have to do with the results of envelopment, or of chemical alteration; to resolve which it becomes necessary to study carefully the problem of envelopment. He then proceeds to show that the enveloped substance is, in some cases, crystalline (and arranged either symmetrically or asymmetrically with regard to the enveloping mass), and in other cases amorphous, and enclosed like the sand-grains which predominate in the calcite crystals of Fontainebleau. The difficulty in deciding whether we have to do with envelopment or with epigenesis increases when the enveloped mineral becomes so abundant as to obscure the enveloping species, or when it becomes mixed with it in so intimate a manner as to seem one with the latter, (*se fondre insensiblement avec lui*). The proportions of the enveloped and the enveloping mineral, we are told, may so far vary that the one or the other is no longer recognizable. "As the forces which determine crystallization have a great energy, the enveloping mineral is sometimes found in so small a quantity as to be entirely masked by the enveloping species." "When minerals have crystallized simultaneously, they have been able to become associated with each other, and to envelop each other, in all proportions" [loc. cit., pages 338, 339, 341, 353].

Our author then proceeds to tell us that having carefully studied, in numerous specimens, the supposed mica-pseudomorphs of iolite, andalusite, cyanite, pyroxene, hornblende, etc., he regards them, as in all cases, examples of envelopment, and expresses the opinion that we must omit from our lists a great number of the so-called pseudomorphous minerals, especially among the silicates. The final result of the process of envelopment is, according to Delesse, this—to give rise to mixed mineral aggregates, owing their external forms to the crystallizing energy of one of the constituents, which may be present in so small a quantity as to be completely obscured by the other matter present. From this condition of things, result crystalline forms which, though totally different in their origin from the products of chemical alteration or substitution, are emphatically pseudomorphs.

From this process of mechanical and more or less heterogeneous envelopment, Delesse next proceeds to consider the crystallizing together of isomorphous or homœomorphous species, in relation to the generally received notion of epigenic pseudomorphism. He declares that "isomorphism explains very well facts which are often attributed to pseudomorphism," and

that many "minerals which are still considered pseudomorphs are in reality examples of isomorphism" [pages 364, 365]. Referring to the well-known investigations of Mitscherlich upon the crystallizing together, in all proportions, of isomorphous species, and of the symmetrical crystallization of one salt around a nucleus of another isomorphous with it, Delesse suggests that the different forms and varieties of hornblendic and pyroxenic minerals afford many examples of the kind. He then adds, "If, as Scheerer has remarked, water plays in silicates the part of a base, anhydrous silicates may crystallize at the same time with hydrated silicates, and moreover be isomorphous with them." In this way, he suggests, we may explain by isomorphism, or homœomorphism, the association with pyroxene of the hydrous species, schiller-spar, as well as that "of various anhydrous and hydrated minerals" [pages 357, 358].

In further illustration of the words just quoted from Delesse, we may cite from Scheerer, as examples of what he called polymeric isomorphism, the association (in the same crystals) of iolite and aspasiolite, and of chrysolite and serpentine. If these, and similar species, crystallize together because they are isomorphous, it is evident that they may each crystallize separately; and thus the crystals of serpentine with the form of chrysolite, and those of aspasiolite and other so-called hydrous iolites, may be regarded as examples not of epigenesis, but of isomorphism.

We have thus endeavored to set forth, chiefly in his own words, the views enunciated in 1859 by Delesse, according to whom the phenomena of so-called pseudomorphism among mineral silicates are to be explained, for the most part, not by chemical alterations of pre-existing species but by envelopment and by isomorphism. That the above are really his views, and are moreover regarded by himself as contrary to those of the school which I oppose, Delesse does not permit us to doubt; for after having set them forth as his own, (*après avoir exposé notre manière de voir*), he says, "We hasten to add that these facts may also be explained in a manner altogether different (*peuvent aussi s'interpreter d'une manière toute différente*); and some savants of Germany, notably G. Rose, Haidinger, Blum, G. Bischof and Rammelsberg have sought their explanation in pseudomorphism. Their example has been followed by most mineralogists, etc." [pages 358, 359].

That the "pseudomorphism" of the authors just named is chemical alteration or epigenesis, it is not necessary to remind the reader; who will now be able to judge whether it is Prof. Dana or myself who has misrepresented or misunderstood Delesse. Let us, however, add that the long and somewhat diffuse memoir of the latter, from which we have quoted, is

wanting in unity of plan and purpose; and that parts of it, if we may hazard a conjecture, seem to have been written while he still inclined to the views of the opposite school. From the table of pseudomorphs which he has given, and from many passages in the text, it might be inferred that he then held the notions of Rose, Haidinger, etc., which he elsewhere, in the same paper, speaks of as being entirely different from his own. The views of Delesse, about this time, underwent a great change, which has a historic importance in connection with those which I advocate. When, in 1857 and 1858, he published the first and second parts of his admirable series of studies on metamorphism, Delesse held, in common with nearly every geologist of the time, to the eruptive origin of serpentine and the related magnesian rocks. Serpentine was then classed by him with other "trappean rocks;" and he elsewhere asserted that "granitic and trappean rocks" undergo in certain cases a change, near their contact with the enclosing rock, by which they lose silica, alumina and alkalies, and acquire magnesia and water, being thus changed into a magnesian silicate; which may take the form of saponite, serpentine, talc or chlorite. [Ann. des Mines, V, xii, 509; xiii, 393, 415]. It would be difficult to state more distinctly the view, which he then held, of the origin of these magnesian rocks and minerals by the chemical alteration of plutonic (granitic and trappean) rocks. This was in 1858, and in 1859 appeared the memoir on pseudomorphs, already noticed, in which, in place of the theory of epigenic pseudomorphism or chemical alteration of various mineral silicates, taught by the German school, he brought forward, in explanation of the facts upon which this was based, another theory, which was only an extension of that already maintained by Scheerer and myself.

It was not until 1861 that Delesse published the last part of his studies on metamorphism, which appeared in the Memoirs of the Academy of Sciences of France (vol. xvii), and in it we find that, consistently with the new views adopted by him in 1859, the old doctrine of the epigenic origin of serpentine and the related magnesian rocks from the alteration of plutonic rocks, is abandoned. In its stead, it is here suggested by Delesse that all these magnesian rocks result from the crystallization of the sepiolites or so-called magnesian clays, which are frequent in many sedimentary deposits. These, according to him, by a molecular re-arrangement of their elements, may give rise to serpentine, talc, chlorite, and their various associated and related minerals. The rocks thus generated are still declared to pass insensibly into plutonic rocks, but instead of maintaining, as in 1858, that they are derived from the latter, Delesse, in 1861, asserts, on the contrary, that "the plutonic

rocks are formed from the metamorphic rocks, and represent the maximum of intensity, or extreme limit of metamorphism."

This recognition of the notion that the great masses of serpentine, with their constantly associated hornblendic, talcose and chlorite rocks, have been directly formed from the molecular re-arrangement or *diagenesis* of aqueous magnesian sediments, and not from the chemical alteration, or *epigenesis* of erupted plutonic masses, marks a complete revolution in our views of the history of the crystalline rocks. The new doctrine did not, however, originate with Delesse, but was previously put forward by myself in a paper "On Some Points of Chemical Geology," read before the Geological Society of London in January, 1859, appearing in abstract in the *Philosophical Magazine* for February, and published at length in the *Geological Journal* for November, in the same year. I there maintained that serpentines were "undoubtedly indigenous rocks, resulting from the alteration of silico-magnesian sediments;" and moreover asserted that the final result of heat, aided by water, on such rocks, would be their softening, and, in certain cases, their extravasation as plutonic rocks; which were regarded "as, in all cases, altered and displaced sediments." When this paper was written, in 1858, I still supposed that the reactions between the elements in beds of siliceous magnesian carbonates (which, I had shown, may give rise to certain magnesian silicates in immediate proximity to eruptive rocks) might serve to explain the origin of great areas of serpentine and related crystalline magnesian silicates; but my studies of the silicates deposited during the evaporation of natural waters, and of the magnesian sediments of the Paris basin, soon led me to seek the origin of these rocks in the alteration of previously formed uncrystalline magnesian silicates. This view was set forth by me in this Journal for March, 1860 [II, xxix, 284] and more fully in the *Canadian Naturalist* for June, 1860 [also in this Journal, xxxii, 286], where it was pointed out that steatite, chlorite and serpentine were probably derived from sediments similar to the magnesian silicates found among the tertiary beds in the vicinity of Paris, the so-called magnesian clays.

We have seen that these various novel views, put forth by me in 1859 and 1860, though totally different from those taught by Delesse in 1858, were integrally adopted by him in 1861. These dates are circumstantially given in my address of last year, and yet Prof. Dana, in his review of it, charges me with "following nearly Delesse" as to the origin of serpentine. He also asserts that I "make Delesse the author of the theory of envelopment," when I have there declared that the view of Delesse—"that the so-called cases of pseudomorphism, on

which the theory of metamorphism by alteration has been built, are, for the most part, examples of association and envelopment, and the result of a contemporaneous and original crystallization,—is identical with the view suggested by Scheerer in 1846, and generalized by myself, when, in 1853, I sought to explain the phenomena in question by the association and crystallizing together of homologous and isomorphous species." To Delesse therefore belongs the merit not of having suggested the notion of envelopment in this connection, but of having pointed out the bearing of the envelopment of heteromorphous and amorphous species on the question before us.

Prof. Dana moreover asserts that while Scheerer is the only one who maintains similar views to myself, I, in common with all other chemists, reject the chemical speculations which lie at the base of his views. On the contrary, unlike most chemists, who have failed to see the great principle which underlies Scheerer's doctrine of polymeric isomorphism, I have maintained [this Journal, II, xv, 230; xvi, 218] that it enters into a general law, in accordance with which bodies whose formulas differ by nM_2O_2 or nH_2O_2, may (like those differing by nH_2C_2), have relations of homology, and moreover be isomorphous. The existence of these same relations was further maintained and exemplified in a paper on Atomic Volumes, read by me before the French Academy of Sciences and published in the *Comptes Rendus* of July 9, 1855. This view, which I have never repudiated, is reiterated in my address last year, and declared to include the polymeric isomorphism of Scheerer.

Prof. Dana next says that in asserting that "the doctrine of pseudomorphism by alteration, as taught by G. Rose, Haidinger, Blum, Volger, Rammelsberg, Dana, Bischof and many others, leads them * * * to maintain the possibility of converting almost any silicate into any other," I have, in his language, "grossly misrepresented the views of at least Rose, Haidinger, Blum, Rammelsberg and Dana," and that I "complete the caricature" by this sentence, to be found in my address: "In this way we are led from gneiss or granite to limestone, from limestone to dolomite, and from dolomite to serpentine; or more directly from granite, granulite or diorite to serpentine at once, without passing through the intermediate stages of limestone and dolomite;"—"part of which transformations," says Prof. Dana, "I, for one, had never conceived; and Rose, Haidinger, Rammelsberg, and probably Blum, and the 'many others,' would repudiate them as strongly as myself." The '*many others*,' as he rightly remarks, are "other writers on pseudomorphism," among whom it would be unjust not to name their progenitor, Breithaupt, von Rath and Müller, at the same time with Volger and Bischof. According to Prof. Dana, I "add to the misrep-

resentation by means of the strange conclusion that because such writers hold that crystals may undergo certain alterations in composition, therefore they believe that rocks of the same constitution may undergo the same changes." This "*strange conclusion*" I have always supposed to be Prof. Dana's own. No one has perhaps asserted it so clearly or so broadly as himself, and I shall therefore quote his own words in my justification. As early as 1845, in an article entitled "Observations on Pseudomorphism," [this Journal I, xlviii, 92] he wrote: "The same process which has altered a few crystals to quartz has distributed silica to fossils without number, scattered through rocks of all ages. The same causes that have originated the steatitic scapolites, occasionally picked out of the rocks, have given magnesia to whole rock-formations, and altered, throughout, their physical and chemical characters. If it be true that the crystals of serpentine are pseudomorphous crystals, altered from chrysolite, it is also true, as Breithaupt has suggested, that the beds of serpentine containing them are likewise altered; though often covering square leagues in extent, and common in most primary formations. The beds of steatite, the still more extensive talcose formations, contain everywhere evidence of the same agents." Again, in 1854, in his *Mineralogy*, 4th edition, [page 226], Prof. Dana, after a complete list of pseudomorphs, compiled from the writers of the school in question, says: "These examples of pseudomorphism should be understood as cases not simply of alteration of crystals, but in many instances of changes in beds of rock. Thus all serpentine, whether in mountain-masses, or the simple crystal, has been formed through *a process of pseudomorphism, or in more general language, of metamorphism;* the same is true of other magnesian rocks, as steatitic, talcose or chloritic slates. *Thus the subject of metamorphism, as it bears on all crystalline rocks, and of pseudomorphism, are but branches of one system of phenomena.*" If there could be any doubt as to the meaning of the words which we have italicized, in quoting them from Prof. Dana, it is removed by his language in 1858. Then, as now, adversely criticising my views on this question, he refers to the statements, above cited, made in 1845 and 1854, as expressions of his doctrine, mentioning especially the first one, in which, he says, "*metamorphism is spoken of as pseudomorphism on a broad scale.*" [This Journal, II, xxv, 445]. I confess that I do not understand Prof. Dana, when in his last criticism of me, fourteen years after the one just quoted, he reproaches me with having charged him with holding the doctrine that "*regional metamorphism is pseudomorphism on a grand scale;*" and declares that he makes no such remark, neither expresses the sentiment in his Mineralogy of 1854.

With these citations before us, and remembering the views Scheerer, and the later ones of Delesse, together with the language of the latter in his essay on Pseudomorphs, let us notice the words of Naumann, addressed to Delesse in 1861, in allusion to the essay in question. "Permit me to express to you my satisfaction for the ideas enunciated in your memoir on Pseudomorphs, ideas which my friend Scheerer will doubtless share with myself" (*idées que mon ami M. Scheerer partagera sans doute comme moi-même*). Then follows the language which I have quoted in my address, in which he combats the error of those who hold that gniesses, amphibolites, and other crystalline rocks are "the results of metamorphic epigenesis, and not original rocks," and adds, "It is precisely *because pseudomorphism has so often been confounded with metamorphism* that this error has found acceptance." [Bull. Soc. Geol. de Fr., II, xviii, 678]. The reader must now judge whose opinions it is that are here denounced as erroneous, and whether Naumann was on the side of Prof. Dana, or, with Delesse, on the side of Scheerer and myself. I insist the more strongly on this matter, because Prof. Dana not only declares that Delesse and Naumann have always avowed the doctrines of the transmutationist school, and do not in any way whatever countenance my views, but implies that I have dealt unfairly with these authorities. In regard to another point raised by Prof. Dana, I may remark that their explicit declarations are not to be set aside, because traces of the doctrine of epigenic pseudomorphism still hold a place in the last edition of Naumann's *Mineralogy*, or in the *Revue de Géologie*, of which Delesse is one of the editors.

Prof. Dana says "If there was any occasion for a notice of my opinions, a critic of 1871 should have referred to the formal expression of them in my *Manual of Geology*, first published in 1863. The reader will there find the *diagenesis* of Gümbel, which Mr. Hunt takes occasion to commend, * * * with but a brief allusion to pseudomorphism." The doctrine of diagenesis, it is hardly necessary to say, I have never attributed to Gümbel, nor does he claim it. It is the old doctrine of Hutton, Playfair and Boué, is taught by Bischof [Chemical Geology, III, 318, 325, 342], and pervades my papers of 1859 and 1860, already referred to. But while it has been generally admitted that what, in my address, I have called the first class of crystalline rocks (consisting chiefly of quartz and aluminous silicates) might result from the molecular re-arrangement of the elements of clay and sand-rocks, I maintained in those papers that what I have called the crystalline rocks of the second class (in which protoxide silicates predominate) have been generated, by a similar process, from deposits of chemically-formed silicates. This view being adopted by Delesse and by Gümbel to explain

the origin of the various magnesian silicated rocks, hitherto generally regarded as the product of epigenesis, the latter has proposed to designate the process as diagenesis; a term which I adopt, as one well fitted to denote the generation of all kinds of crystalline rocks through a molecular re-arrangement of sedimentary deposits, of whatever origin. Prof. Dana, in common with most other geologists, admits in his *Manual* the production by diagenesis of the rocks of the first class, but in the case of serpentine and steatite declares them to have been formed by epigenic pseudomorphism or chemical alteration of pyroxenic and other crystalline rocks; *the origin of which is left entirely unexplained.* It is true that his allusions to pseudomorphism in this volume are confined to very brief notices, on pages 704 and 710; a fact which is the more noticeable, when we recall that the author had formerly expressed the belief "that pseudormorphism will soon constitute one of the most important chapters in geological treatises." [This Journal, I, xlviii, 66.] That Prof. Dana has receded from the extreme views on this subject which he maintained from 1845 to 1858, and which I have constantly opposed, seems probable; but until he formally rejects them, the student of geology will not unnaturally suppose that he still gives the sanction of his authority to the doctrine which he once taught, without any qualification, but now repudiates, that "*metamorphism is pseudomorphism on a broad scale.*"

Prof. Dana having clearly defined the proposition that the chemical alterations which are recognized in individual crystals are to be conceived as extending to rock-masses; and having moreover asserted that the principle of the identity of metamorphism and pseudomorphism "bears on all crystalline rocks," is logically committed to all the deductions as to the changes of rocks which the transmutationist school has drawn from the supposed alteration of minerals. By reference to the table of pseudomorphs in the fourth edition of Dana's Mineralogy, it will be seen that each one of the metamorphoses of rocks mentioned in the above extract from my address is based upon an asserted epigenic change or conversion of the constituent species. I shall however show, in addition, that in each case the application of the principle to rock-masses has been recognized by one or more of the authorities already named, and that the so-called caricature has been drawn by their own hands. It would be easy, did space permit, to extend greatly this list of supposed transmutations. The various associations of rocks and minerals in nature, when interpreted according to the canons of this school, seem in fact, as remarked by Prof. Warrington Smyth, in his address already quoted, "to offer a premium to the ingenious for inventing an almost infinite

series of possible combinations and permutations." Before proceeding further it is to be noted that no distinction can, in many cases, be established between the results of alteration (or partial replacement) and substitution (or complete replacement); since successive alterations may give the same product as direct substitution. Thus, for example, quartz might be directly replaced by calcite, or else first altered to a silicate of lime, which, in its turn, might be changed to carbonate. The alteration of quartz to a silicate of magnesia, and that of both pyroxene and pectolite to calcite, is maintained by the writers of the present school.

Metamorphosis of granite or gneiss to limestone. Calcite, we are told, is pseudomorphous of quartz, of feldspar, of pyroxene, and of garnet, besides other species: it moreover replaces both orthoclase and albite "by some process of solution and substitution." [Dana's Mineralogy, 5th edition, 361.] Since quartz, orthoclase and albite can be replaced by calcite, the transmutation of granite or gneiss into limestone presents no difficulty. I cannot, at present, give the reference to the statement of Volger that some gneissoid limestones owe their origin to such a process.

Metamorphosis of limestone to dolomite. This change is maintained by von Buch, Haidinger and many others. I am blamed for mentioning in connection with this school the name of Haidinger, who, Prof. Dana says, "never wrote upon the subject of the alteration of rocks." It has, however, never before been questioned that Haidinger was the first, if not to suggest, to clearly set forth the theory of the supposed conversion of limestone into dolomite by the action of magnesian solutions, aided by heat and pressure; a theory which I have elsewhere refuted. [Bischof, Chem. Geology, iii, 155, 158; Zirkel, Petrographie, i, 246; Liebig and Kopp, Jahresbericht, 1847–48, 1289, and this Journal, II, xxviii, 376].

Metamorphosis of dolomite to serpentine. This change is maintained by G. Rose [Bischof, Chem. Geol., ii, 423], and by Dana [this Journal, III, iii, 89].

Metamorphosis of granite, granulite, and eclogite directly into serpentine, chlorite and talc. These transmutations are maintained by Müller, and adopted by Bischof. [Chem. Geol., ii, 424, 434.]

Metamorphosis of limestone to granite or gneiss. This is taught by Blum and Volger. [Ibid., ii, 186; iii, 431.]

Having thus given the authorities for the examples cited in my address, I may notice some further illustrations of the doctrine from the pages of Bischof's work already quoted. Metamorphosis of diorite, hornblende-rock and labradorite to serpentine; G. Rose, Breithaupt, von Rath [ii, 417, 418]. Diorite and

hornblende-slate to talc-slate and chlorite-slate; G. Rose [iii, 312]. Mica-slate to talc-slate, and steatite and mica to serpentine, steatite and talc; Blum, C. Gmelin [ii, 405, 468]. Quartz-rock to steatite; Blum [ii, 468].

With regard to New England rocks, Prof. Dana asserts that "there are gneisses, mica-schists, and chloritic and talcoid schists in the Taconic series." I have, however, shown in my address that Emmons, the author of the Taconic system, expressly excluded therefrom the crystalline rocks, which he included in an older primary system; excepting, however, certain micaceous and talcose beds, which he declared to be recomposed rocks, made up from the ruins of the primary schists, and distinguished from these by the absence of the characteristic crystalline minerals which belong to the Green Mountain primary schists.

Again, Prof. Dana states that I make the crystalline schists of the White Mountains a newer series than the Green Mountain rocks. A careful perusal of my address will show that I nowhere assert that the rocks of the third series, on my line of section, are younger than the second series. Such a view of their relations has, however, been maintained for the last generation by the Messrs. Rogers, Logan, and many others, all of whom assigned the crystalline schists of the White Mountains to a higher geological horizon than the Green Mountains. In support of this view of their relative antiquity, I have, it is true, brought together observations from South Carolina, Pennsylvania, Michigan, Ontario, and Maine, all of which point to the same conclusion; and I might now add similar evidence from New Brunswick and from Nova Scotia. My "chronological arrangement" of New England crystalline rocks, as it is called by Prof. Dana, so far as it is my own, is limited to my affirmation that they are all of pre-Cambrian age; in proof of which it need only be mentioned that the crystalline schists of both the types in question are, in southern New Brunswick, directly overlaid by uncrystalline shales, sandstones and conglomerates, made up in part of the ruins of these, and holding a Cambrian (Menevian) fauna.

As regards the mica-schists with staurolite, cyanite, andalusite and garnet, I have in my address pointed out the fact that they appear to belong to a great series of rocks, very constant in character, which have a continuous outcrop from the Hudson river to the St. John, a distance of 500 miles, and, in the latter region are clearly pre-Cambrian. I have moreover brought together the evidence of observers in other parts of North America, in Great Britain, in continental Europe, and in Australia, showing that similar crystalline schists, holding these same minerals, always occupy, in these regions, a similar geological horizon. Prof. Dana hereupon inquires whether any

one has yet *proved* that these mineral characters are restricted to rocks of a certain geological period. I answer, that in opposition to these facts, it has not yet been proved that they belong to any later geological period than the one already indicated; and that it is only by bringing together observations, as I have done, that we can ever hope to determine the geological value of these mineral fossils. In no other way did William Smith prove, in Great Britain, the value of organic fossils, and thus lay the foundations of paleontological geology.

Montreal, April, 1872.

ART. XI.—*On the Meteors of April 30th–May 1st;* by Professor DANIEL KIRKWOOD.

PROFESSOR SCHIAPARELLI, in his list of meteoric showers whose radiant points are derived from observations made in Italy within the last few years, describes one as occurring on April 30th and May 1st, the apparent position of whose radiant is in the Northern Crown, R. A. 237°, N. P. D. 55°. The same shower has also been recognized by Robert P. Greg, F.R.S., of Manchester, England. This meteor-stream, it is now proposed to show, is probably derived from one much more conspicuous in ancient times.

In Quetelet's *Physique du Globe*, pp. 290–297, we find meteoric displays of the following dates. In each case the corresponding day for 1870 is also given,* in order to exhibit the close agreement of the epochs.

1.	A. D. 401,	April 9th;	corresponding to	April 29th,	for 1870.
2.	" 538,	" 6th;	"	April 25th,	"
3.	" 839,	" 17th;	"	May 1st,	"
4.	" 927,	" 17th;	"	April 30th,	"
5.	" 934,	" 18th;	"	May 1st,	"
6.	" 1009,	" 16th;	"	April 28th,	"

The epochs of 927 and 934 suggest as probable the short period of 7 years. It is found accordingly that the entire interval of 608 years—from 401 to 1009—is equal to 89 mean periods of 6·8315 years each. With this approximate value the six dates are all represented as follows:

From A. D. 401 to	A. D. 538,	20	periods of	6·85 years.
" 538 to	839,	44	"	6·84 "
" 839 to	927,	13	"	6·77 "
" 927 to	934,	1	"	7·00 "
" 934 to	1009,	11	"	6·82 "

This period corresponds approximately to those of several comets whose aphelion distances are somewhat greater than the

* Making proper allowance for the precession of the equinoxes.

mean distance of Jupiter. So long as the cluster occupied but a small arc of the orbit, the displays would evidently be separated by considerable intervals. The two *consecutive* showers in the tenth century indicate, however, an extensive diffusion of the cluster at that epoch; so that the *preceding* part passed the node April 30th, 927, and the *following* part May 1st, 934. The comparative paucity of meteors in modern times may be particularly explained by the fact that the ring has been subject to frequent perturbations by Jupiter.

It is not impossible that this meteor-stream was connected in its origin with the comet which passed its perihelion about the 29th of April, B. C. 136.

ART. XII.—*On the Tertiary Basin of the Marañon;* by CH. FRED. HARTT, A.M.

ON the 12th of December, 1867, Prof. James Orton of Vassar College, on his journey down the Marañon, or Peruvian Amazonas,* spent a few hours at Pebas, a little village on the Ambayacú a mile from the left bank of the Marañon, and some 50–60 miles below the mouth of the Napo. Here he collected several species of fossil shells, but strangely neglected to observe the mode of their occurrence. In announcing his discovery in the Geological Magazine,† he says that "the shells are all found in the colored plastic clays which stretch unbroken from the foot of the Andes to the Atlantic." In the American Journal of Conchology,‡ he speaks of them as occurring "in that peculiar formation of fine laminated colored clays which is spread over the entire valley of the great river, and which Prof. Agassiz had

* As this river is called by some writers the Amazon, Rio Amazon, Amazons or Rio Amazons, I have preferred to use the Portuguese form. If one uses either Amazon or Amazons there is no propriety in prefixing the Portuguese word Rio. The name of the river is Rio das Amazonas, the river of the Amazons; but in Brazil it is commonly spoken of as *o Amazonas* or the Amazons. I have simply followed the rule of not attempting to translate South American names. The name Marañon, about which there has long been so much discussion, is evidently the same as the Tupí word *paraná*, which means *a river*; Maranhão or Maranham is the Portuguese form for Marañon. M and p are interchangeable consonants, as may be seen in a great number of words in modern Lingua geral, as *morandú* or *porandú* to question, *puraçéi* or *muraçéi* to dance, etc. The terminal vowel of *paraná* is often more or less nasal; hence the Brazilian geographical names *Paranán*,, *Juparanda*. That Marañon or Maranhão (Maranyáw$_G$) are derived from paraná (or maraná ?) is all the more probable as this is the name given by the Indians of the Amazonas, and not only by those who speak modern north Tupí or Lingua geral, but also the Omagua, Cocáma and other languages. Ask an Indian who does not speak Spanish or Portuguese the name of the Amazonas, and he answers *Paraná*. But to ears unaccustomed to the language it may sound like Marañon.

† Henry Woodward. The Tertiary shells of the Amazons Valley, from Ann. and Mag. Nat. Hist. for Jan. and Feb., 1871, p. 6.

‡ *Conrad*, Desc. of new fossil shells of the Rio Amazon, published in advance, Oct. 10, 1870.

pronounced drift;" while in his "Andes and Amazon" he simply says that the fossils occurred in a fossiliferous bed intercalated between the variegated clays so peculiar to the Amazon,"* and that "interstratified with the clay deposits are seams of a highly bituminous lignite.†" Prof. Orton therefore leaves it to be inferred that the Pebas beds are traceable down the whole length of the Amazonas. Mr. Henry Woodward, in the paper just quoted in a foot note, says that the Pebas clays are "evidently Bed II. of Prof. Agassiz's section.‡" Prof. Orton submitted his fossils to Mr. Gabb, who described and figured them§ under the names of *Neritina pupa, Turbonilla minuscula, Mesalia Ortoni, Tellina Amazoniensis, Pachydon obliqua* and *P. tenua.* In Mr. Gabb's opinion these remains indicated a fauna of Tertiary age. On the strength of this opinion Prof. Orton ventured to attack Prof. Agassiz's theory of the glacial origin of the valley of the Amazonas, laying stress on the fact that the shells occur well preserved, in place, and "showing" no indication of a "grinding glacier."

Under the instructions of Prof. Orton, Mr. Hauxwell, an intelligent naturalist, resident some 30 years on the Amazonas, made larger and more complete collections of these shells and found the fossiliferous beds elsewhere on the Marañon, especially at Cochaquinas on the southern side of the river. These collections were placed in the hands of Mr. Conrad, who described them, distinguishing ten species of gasteropods and six of lamellibranchs, referring all the latter to the genus *Pachydon* (*Anisothyris* Conrad). More recently Mr. Hauxwell sent large collections to England. Those in the possession of Mr. Janson of London were examined by Mr. Henry Woodward of the British Museum, and form the subject of the paper already twice referred to. Mr. Woodward makes several changes of nomenclature, and describes two new species. The list of the Pebas fossils now stands as follows:

GASTEROPODA.

Isæa Conrad (freshwater ?)
I. Ortoni Conrad=*Mesalia Ortoni* Gabb.
I. lintea Conrad.
Liris Conrad (freshwater ?)
L. laqueata Conrad.
Ebora (freshwater or marine ?)
E. crassilabra Conrad.
Nesis (sub-genus of *Ebora*).
N. bella Conrad.
Hemisinus Swainson (freshwater !)
H. sulcatus Conrad.
Dyris Conrad.
D. gracilis Conrad.
Neritina Lamarck (fresh and brackish water).
N. pupa Gabb (=*N. Ortoni* Conrad ?)
Bulimus Scopoli (land).
B. linteus Conrad.
Turbonilla Risso.
T. minuscula Gabb.
Odostomia ? Woodward (brackish water).

* Andes and Amazon, 282.
† Op. cit., 283.
‡ Bull. de la Soc. Géol. de France, 2d Série, T. xxv, p. 685. Hartt, Geol. and Phys. Geog. of Brazil, p. 487.
§ Amer. Jour. Conch., vol. iv.

LAMELLIBRANCHIATA.

Anodon Cuvier (fresh water).
Anodon Batesii Woodward.
Anisothyris Conrad (=*Pachydon* Gabb) (brackish water).
A. tenuis (=*Pachydon tenuis* Gabb, *P. tenuis* Conrad, *Anisothyris Hauxwelli* Woodward);* of this species Mr. Woodward distinguishes two varieties: *a, distorta*, *β, crassa*.
A. ovata Conrad.
A. carinata Conrad.
A. obliqua Conrad.
A. erecta Conrad.
A. cuneata Conrad.
Tellina.
T. Amazoniensis Gabb.

In the summer of 1871 I met Mr. J. B. Steere, a graduate of Michigan University, who was traveling on the Amazonas, making natural history collections. We spent more than a month together, and I took him over my old ground at Ereré and Monte Alegre. As he was about to visit the upper Amazonas, I gave him instructions to examine the Pebas locality, make a geological section, showing the character and arrangement of the beds, and collect carefully the fossils. Under date of Jan. 26th of this year, he has written me an account of his visit to the locality in question, and has sent some interesting notes which give us for the first time a clear idea of its geological structure, and of the conditions under which the fossils are found.

Mr. Steere says that a short distance below Tabatinga,† which, it will we remembered, is just on the boundary line separating Brazil from Peru, he saw "horizontal beds of blue clay, with veins of clayey coal dividing them. These veins of coal seem to vary much in thickness and appearance in a distance of a few hundred yards." This series of deposits he claims to be the same as that affording fossils at Pebas. My good friend Dr. Pimentel, Major in the Brazilian army and one of the engineers on a late Brazilio-Peruvian Boundary Commission, stationed at Tabatinga, had previously found, just above this town, a heavy bed of lignite, specimens of which he sent me.

Mr. Steere first saw signs of fossils in clay beds just above Loreto, a little place on the left bank of the Marañon some 30 miles above Tabatinga; but owing to the shortness of the stop of the steamer, he was unable to examine the locality with care. He describes the country below Pebas as low and less than a hundred feet above river level, i. e., during the dry season. The fossiliferous clay beds lie near the level of the river, but they are covered by 20–30 feet of red clay which he compares to the superficial clays so common on the lower Amazonas. Pebas, as already stated, is situated on the left bank of the Rio Am-

* I sympathize with the wish to show honor to so deserving a gentleman as Mr. Hauxwell, but the change of the specific name from *tenuis*, however inappropriate the term may be, to *Hauxwelli*, is unwise and inadmissible.

† Tabatinga is the name given to the white feldspathic clay common all over Brazil. *Toud*, Tupí, *taba* Portuguese form, is a yellowish clay; *tinga* means white.

bayacú, a mile above its confluence with the Marañon. Two miles below the mouth of the Ambayacú is Old Pebas. Both sites are on the high *tierra firme*.* The right bank of the Amazonas opposite the Ambayacú is recent and low, but farther down the *tierra firme* appears, and Pichana is situated upon it.

The bank on which Pebas stands, Mr. Steere says, is about 100 feet high, that is during the dry season. In front of the village the lower strata are hidden from view by a slope of rubbish, but the upper are quite well exposed. The following section was made by Mr. Steere in a ravine near the road leading up the bank. The order is ascending.

I. The lowest bed seen is a blue clay of which a thickness of fifteen feet is uncovered. In the middle is a band three feet in thickness containing shells.

II. A well defined seam of lignite, six inches in thickness. For a few inches above and below this, the clay is filled with vegetable remains.

III. A bed of blue clay, thirteen feet in thickness, with an occasional shell too badly preserved to be removed.

IV. Blue clay, five feet thick, and full of fossils.

V. A bed, ten feet in thickness, of red and white clay, and sand, without fossils. This forms the surface deposit.

Not far from the ravine where the first section was made, Mr. Steere made another as follows:

I. 2 or 3 ft. of clay full of fossils.
II. 10 ft. blue clay.
III. 3 ft. blue clay filled with fossils.
IV. 5 ft. dirty coal.
V. 5 or 6 ft. of red and white clay.

In a ravine in the forest near the village, he made still another section, "finding in descending order" (I quote his own words) "five or six feet of red and white clay; a vein of dirty coal (two feet); blue clay without fossils, ten feet; another narrow vein of coal; eight or ten feet of blue clay without fossils; more coal; beds of clay without fossils; more coal veins."†

Mr. Steere visited Pichana, where he found much the same structure as at Pebas. At Old Pebas the same beds are seen containing beds of lignite, but they appear to be more denuded than at New Pebas.

At Iquitos Mr. Steere found similar beds that appeared to be the continuation of those of Pebas, but afforded no fossils.

* Land not laid under water during the annual flood.

† Lieut. Herndon visited Pebas in 1851. He speaks of the ravines back of the town in which a black slate rock crops out, and says that he brought from the old town to the new "specimens of black clay slate, that crops out in narrow veins on the bank, and made a fire with it, which burned all night, with a strong bituminous smell."—Exploration of Valley of the Amazonas, Pt. I, pp. 219–220.

Mr. Steere has made very extensive collections of the fossils of the Pebas locality and vicinity, and they will probably afford some new species. When these collections with their accompanying lithological specimens shall have been studied, we shall have more details relative to the character of the beds and probably some facts bearing on the vertical distribution of the species. Mr. Steere simply says that the bivalves are more numerous in the lower and the univalves in the upper beds.

In examining the above sections, we find the surface bed always composed of variegated clays, with more or less sand, which, according to Mr. Steere were deposited on the much denuded surface of the lower fossiliferous beds, the last being clearly Tertiary. We have then at Pebas two well marked formations to deal with. It seems to me a little doubtful whether Prof. Agassiz could have seen the blue clays in the neighborhood of Tabatinga, for he makes no mention of the lignites which occur in them, and it also seems to me doubtful whether the greyish laminated clays with leaves, at Tonantins, which correspond with the iron stone with similar leaves, found by me on the Tapajos, can belong to the same formation. It is therefore by no means clear that Prof. Agassiz included any part of the Pebas series in his theoretical section of the Amazonian valley. I see no reason for referring the Pebas Tertiary to Bed II. of Prof. Agassiz's section. In sooth, that section is simply theoretical and cannot stand. There is no resemblance whatever between the blue Tertiary clays of Pebas and the mottled or variegated clays of the vicinity of Pará. The surface clays of Pebas, however, appear to resemble them; but resemblance in lithological structure, color, etc., is not identity, for one may find "variegated clays" on the Amazonas containing Devonian or Carboniferous fossils.

The Pebas fossils do not therefore occur in the superficial variegated clays, but in an older and distinct underlying formation quite unlike the ordinary more recent variegated clays of the Amazonas. The fossils, therefore, aid us only in determining the age of the lower series, leaving the question of the age of the superficial clays undecided. They certainly afford no proof that these latter clays are not of glacial origin. As to the condition under which these superficial clays were deposited, we simply have so far no evidence whatever, although the probabilities are that they are of quite modern origin; but whether fluviatile or estuarine, is a question which cannot be settled till we know more about their distribution. I dare not compare them with the superficial clays of the Lower Amazonas, for my experience with these deposits has satisfied me, that, how similar soever these beds may be in different localities, they may

vary in age and greatly in the conditions under which they were deposited.

The Pebas shells do not shed one ray of light on the great question of the glaciation of the Amazonian valley. I have, however, shown that the supposed facts on which Prof. Agassiz founded his theory, viz: the assumed identity of structure of the Serras of Ereré and Parú (Almeyrim); the occurrence of erratics of diorite at Ereré, etc., were no facts at all. Ereré is a monoclinal ridge of sandstone which no geologist would ever think of calling drift, and the supposed drift clays at its base contain lower Devonian trilobites and are traversed by trap dykes; the supposed erratics of diorite are *boulders of decomposition;* the Serras of Parú* are composed of horizontal beds of soft rocks undoubtedly more modern than the Serra of Ereré and offering not the first evidence of glacial origin; the gigantic moraine which Prof. Agassiz thought to have extended across the mouth of the Amazonas does not exist. Moreover I have failed in finding, during many months of careful search, anything like drift in the province of Pará; and therefore, having no evidence whatever of the former existence of glaciers in the Amazonas, the question of the glacial origin of the valley need not be raised.

While I do not believe in the glaciation of the Amazonas, I still adhere to the belief that glaciers have existed in the central and southern portions of the Brazilian plateau. Prof. O. H. St. John, who, as one of the geologists of the Thayer expedition, made a journey through the interior of Brazil from Rio de Janeiro to Maranhâo, assures me that he has found not only the superficial deposits, but also the topography characteristic of a glaciated country in Minas Geraes, while these phenomena are not visible in Piauhy and Maranhâo.

Though the Pebas shells throw no light on the question of the glaciation of the Amazonian valley, they aid in establishing the fact that the Upper Amazonas or Marañon, from Iquitos to Tabatinga, a distance of some 240–250 miles, flows through a Tertiary basin, the channel of the river being deeply cut through beds of this age. The width of this basin is undetermined, as is also the exact age of the beds, for the nature of the fauna is such that it is impossible to say to which division of the Tertiary they are to be referred. The fauna indicates an estuary formation. That at the time of the deposition of Pebas beds there was water communication between the basins of the Amazonas and the Orinoco is scarcely probable.

* I visited the Serra of Parauáquára in 1871.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On the light emitted by the vapor of iodine.*—Salet has found that the vapor of iodine may be heated to redness like a solid or liquid. It then emits the less refrangible luminous rays which furnish a continuous spectrum. The experiment is easily made by heating the iodine in a tube of Bohemian glass. A small crystal of iodine is placed in a tube of thick glass, which is then heated strongly at some distance from the crystal. When the glass is red for a considerable part of its length, it is to be allowed to cool until it is no longer visible in the dark; the iodine is then to be rapidly volatilized. The colored vapor reaching the heated part of the tube then glows with a distinct red light. This experiment shows that the iodine becomes luminous at a lower temperature than glass. Another method of exhibiting the incandescence of the vapor of iodine is the following: A spiral of fine platinum wire is sealed in the interior of a tube of glass eight millimeters in diameter. Pure iodine is then introduced into the tube, which after expulsion of the air is sealed. If the iodine be then volatilized and the wire ignited by a battery, the spiral appears surrounded by a flame of a very rich red color, which yields the well-known interrupted spectrum.—*Comptes Rendus*, Tome lxxiv, p. 1249. W. G.

2. *On the absorption spectra of the vapors of selenium and of certain other bodies.*—Gernez has found that the vapors of selenium and a number of other colored vapors give distinctly marked absorption bands. When selenium is heated in a porcelain tube closed at its extremities by plates of glass, we observe a progressive absorption of all the rays of the spectrum, beginning with the most refrangible and proceeding toward the red, but without any traces of dark bands. But if we continue to raise the temperature, the tint of the vapor becomes more clear and the different regions of the spectrum reappear, crossed in the blue and the violet by bundles of dark bands. The result cannot be ascribed to the formation of selenious acid, since the same phenomena are observed when the selenium is heated in an atmosphere of dry carbonic acid. Selenious chloride, which is a brown limpid liquid, gives a vapor the spectrum of which is crossed by rays beginning at the limit of the green and the blue, and extending to the extremity of the violet. Bromide of selenium produces systems of almost equidistant rays, but in a different part of the spectrum not specified by the author. Tellurium when volatilized in an atmosphere of dry carbonic acid gas emits at a very high temperature a golden yellow vapor, which yields a very brilliant absorption spectrum much more extended toward the red than the spectra of sulphur and selenium, and composed of systems of fine rays extending from the yellow into the violet. Tellurous chloride gives yellow vapors, which act very strongly on light. The absorption spectrum is particularly developed in the orange and the green. Tellurous

bromide gives a violet vapor, the most remarkable absorption bands of which are in the red and the yellow. Proto-bromide of iodine gives off at ordinary temperatures vapors which in thin layers have a copper-red color. The absorption spectrum of this substance presents very fine rays in the red, yellow and orange; and differs from the spectrum obtained by passing light through successive layers of iodine and bromine vapors. Alizarin when carefully heated gives vapors which exhibit systems of sensibly equidistant rays in about the middle of the spectrum.—*Comptes Rendus*, Tome lxxiv, p. 1190. W. G.

3. *On the absorption spectra of the vapors of sulphur, selenious acid and hypochlorous acid.*—The same writer has observed the existence of dark lines in the spectra of many other colored vapors. The source of light employed was the Drummond light so-called. Sulphur heated in a porcelain tube closed at the extremities with plates of glass gave at first vapors which absorbed the most refrangible rays of the spectrum, leaving finally a red band extending a little beyond C. On raising the temperature higher this band spreads out; the other rays of the spectrum then reappear, the violet and blue being crossed by bundles of dark rays. The phenomenon is therefore the same as that observed in the case of selenium. Selenious acid at the instant of vaporization gives well-marked dark lines, especially in the violet and blue. The author found the absorption spectrum of hypochlorous acid identical with that of hypochloric and chlorous acids, only in the case of hypochlorous acid the layer of gas must be much longer in order that the phenomenon may be distinctly visible. Aqueous solutions of all these gases give the most salient lines of the gases themselves. —*Comptes Rendus*, Tome lxxiv, p. 803. W. G.

4. *On fluoride of silver.*—In continuing his researches on the compounds of fluorine, Mr. G. Gore has arrived at the following results: Iodine acting upon argentic fluoride with the aid of heat produces argentic iodide and fluoride of iodine. Fluoride of iodine is a volatile, colorless liquid, which does not corrode mercury or red-hot platinum, but which corrodes glass at 60° F., and crystals of silicon at a red heat, as well as platinum in contact with argentic fluoride in a state of fusion. It fumes strongly in the air, and is decomposed by water into fluohydric and iodic acids,

$$IF_5+3H_2O=5HF+HIO_3.$$

By heating argentic fluoride to redness in a current of dried, cool gas, it was wholly reduced to metallic silver. Fluohydric acid and carbonic tetro-fluoride being evolved, vitrified boric acid violently decomposed the fluoride when fused, emitting copious white acid fumes, but did not act on an aqueous solution of the salt at 60° F. Crystals of silicon placed upon argentic fluoride when fused became at once red hot, undergoing rapid combustion, and evolving fluoride of silicon. A lump of fused silicon also slowly decomposed an aqueous solution of argentic fluoride, setting free metallic silver in crystals. Crystals of silicon behaved in the same manner, but much more rapidly, and evolved abundance of gas if the solution contained free fluohydric acid; on adding nitric acid

to this mixture, bubbles of spontaneously inflammable silicide of hydrogen were evolved and ignited. Pure and dry silicon added to argentic fluoride at a temperature of low redness evolved much heat with violent action, and set free metallic silver. The fluoride when fused is rapidly decomposed by sulphur with evolution of heat, fluoride of sulphur and argentic sulphide being formed. Fluoride of sulphur is a heavy, colorless vapor, not condensing at 0° C. and 760^{mm}. It corrodes glass, fumes strongly in the air, and has a very powerful dusty odor. Sulphur rapidly decomposed an aqueous solution of argentic fluoride. When the vapor of carbonic disulphide is passed over argentic fluoride at a red heat, argentic sulphide and carbonic tetra-fluoride are formed, the reaction being represented by the equation:

$$4AgF + CS_2 = 2Ag_2S + CF_4.$$

The tetra-fluoride is a fuming acid vapor.—*L. and E. Phil. Mag., May*, 1872. W. G.

5. *On a method of fixing the Constitution of Acids and Alcohols by the oxidation of their Ketones.*—In his researches upon the oxidation of ketones, POPOFF observed a uniformity of results which led him to suggest this oxidation as a means of determining the rational constitution of acids and alcohols. Taking the four varieties of ketone expressed by the following general formulas:—

$$\begin{array}{lll} (1) & CH_3 - (CH_2)_n - CO - R \\ (2) = CH & - (CH_2)_n - CO - R \\ (3) & = CH & - CO - R \\ (4) & \equiv C & - CO - R \end{array}$$

—in which R represents one of the alcohol-radicals united directly to the carbonyl, generally methyl, phenyl, or ethyl—Popoff finds that by oxidation, the carbonyl remains combined with the radical R, while the other alcohol radical is oxidized. If this other radical be that of a normal alcohol—as is the case in the first of the above formulas—then a normal acid results; if it be an iso-alcohol radical,—as in the second formula—an iso-acid results; a secondary-alcohol radical,—as in the third formula—gives an acetone; while a tertiary-alcohol radical—as in the last formula—splits up by oxidation. Now, since the ketones can be prepared from the fatty acids,—either by distillation of a suitable mixture of their salts or treating the acid chloride with the zinc-compound of the desired alcohol-radical—and since these ketones when submitted to oxidation, yield their carbonyl combined with the alcohol-radical R, while the other alcohol-radical—which is furnished by the acid—is oxidized after the manner just given, it is clear that this method may be used to determine the constitution of the alcohol-radical which is contained in any acid. Moreover, since the fatty acids may be obtained by oxidizing the corresponding alcohols, the constitution of these latter may also be determined by this method.

To test the method, amyl alcohol—of boiling point 130° to 131·5°, and whose power of rotation in a tube 25 cm. long was −2·4°,—

was oxidized, and the valeric acid produced—which boiled from 174° to 176°, and in which $\alpha = +4{\cdot}4^{\circ}$,—was converted into the calcium salt, and distilled with an equivalent quantity of calcium benzoate. On rectifying the product, it boiled at 225° to 226°, and afforded on analysis numbers agreeing with those required by butylphenyl ketone. To fix the constitution of the butyl it contained, the ketone was oxidized. The products consisted of benzoic acid and iso-butyric acid, with traces of acetic acid; thus proving that this ketone, and also, therefore, the valeric acid and the amyl alcohol from which it was derived, contain iso-butyl and not butyl. Their constitutions respectively are—

$$\begin{matrix}CH_3\\CH_3\end{matrix}\!>\!CH\!-\!CH_2\!-\!COOH \text{ and } \begin{matrix}CH_3\\CH_3\end{matrix}\!>\!CH\!-\!CH_2\!-\!CH_2OH,$$

a result already confirmed in other ways by Erlenmeyer, Frankland and Duppa, and Butlerow.—*Ber. Berl. chem. Ges.*, v, 38, Feb., 1872. G. F. B.

6. *On Phenol-colors and their Relation to Natural Coloring Matters.*—Baeyer has continued his researches on gallein and fluorescein,* and other coloring matters derived from the phenols. He finds that the phthalic acid does not act solely by abstracting water, as he at first supposed; but that it enters itself into the composition of the new molecule. He finds, moreover, that generally, whenever the phenols are heated with polybasic organic acids, either alone or in presence of glycerin or sulphuric acid, water is abstracted, and a series of compounds, not ethers, is formed. Some of these are indifferent bodies, while others are soluble in potassium hydrate with an intense color, and on reduction become colorless. These latter, many of them, give, when heated with sulphuric acid, new colored compounds, which differ from them in the fact that, when reduced, the products are also colored. Baeyer proposes to terminate the name of the former class of bodies, which are soluble in potassium hydrate with color, with the syllable *-ein*, while to the colorless bodies obtained from these by reduction he gives names terminating in *-in*. The indifferent bodies he calls anhydrides of the former. Of the almost unlimited number of bodies thus possible, the author describes:—

(1.) Phenol colors. When 10 parts of phenol, 5 parts phthalic oxide, and 4 parts concentrated sulphuric acid are heated to 120° to 130° for several hours, a red mass is obtained, yielding a yellowish-white powder on treatment with boiling benzol. When dissolved in potassium hydrate, and precipitated with hydrochloric acid, a granular precipitate is obtained, having the composition $C_{20}H_{14}O_4$. It is the phthalein of phenol has probably the rational constitution $C_6H_4(CO.C_6H_4OH)_2$, and is isomeric with the phthalic ether of phenol. When heated, in solution in potassium hydrate, with zinc-dust, the magnificently fuchsine-colored solution is decolorized, and hydrochloric acid precipitates therefrom the white granular phthalin of phenol, $C_{20}H_{16}O_4$. Mellitic and pyromellitic acids act similarly upon phenol; but the most inter-

* This Journal, III, ii, 203.

esting action is that of oxalic acid, which has long been known, and the product of which is rosolic acid. The aurin, lately isolated from rosolic acid by Dale and Schorlemmer, Baeyer supposes to be $C_{25}H_{20}O_8$ and to result from the oxidation of leucoaurin $C_{25}H_{20}O_4$, which is thus produced:

$$CO_2+(C_6H_6O)_4=C_{25}H_{20}O_4+(H_2O)_2.$$

(2.) α Naphthol colors. α Naphthol, heated with phthalic oxide, yields light yellow crystals of the anhydride of the phthalein of naphthol, $C_{28}H_{16}O_3$, insoluble in potassium hydrate. Heated with sulphuric acid, it gives a beautiful red body, $C_{28}H_{18}O_8$. Oxalic, mellitic and pyromellitic acids act similarly on α naphthol.

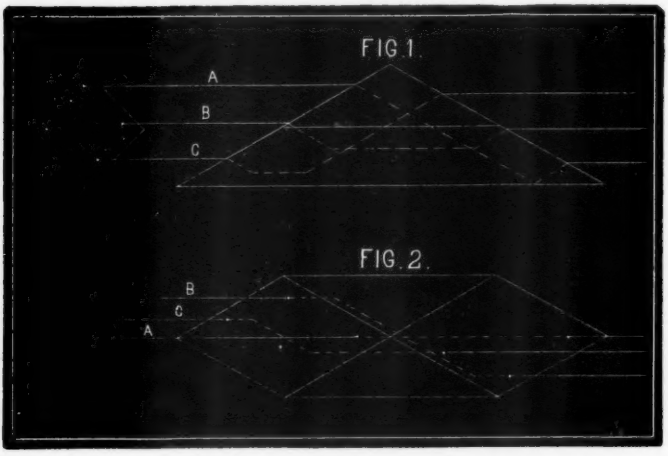
(3.) Resorcin colors. Resorcin heated with phthalic oxide, gives the phthalein of resorcin, or fluorescein, which, precipitated from its potash solution by hydrochloric acid, is $C_{20}H_{14}O_6$, but recrystallized from alcohol is $C_{20}H_{12}O_5$. Reduced by zinc-dust, the corresponding phthalin is obtained. Heated with sulphuric acid, a red body is formed, which is turned blue by alkalies, and which yields a second red body on reduction. It closely resembles the coloring matter of litmus. Succinic oxide gives with resorcin, the succinein of resorcin; and oxalic acid, the carbonein, or euxanthon, $C_{13}H_8O_4$.

(4.) Pyrogallol colors. Pyrogallol (pyrogallic acid) by the action of phthalic oxide, gives gallein, $C_{20}H_{12}O_7$, the phthalein of pyrogallol. Reduced, it gives gallin $C_{20}H_{18}O_7$. Heated with sulphuric acid, it forms cœrulein, $C_{20}H_{10}O_7$, and this on reduction gives cœrulin. Oxalic acid and succinic oxide, as well as oil of bitter-almonds, acetone, etc., also afford colored compounds when heated with pyrogallol.

Hydroquinone gives with phthalic oxide a red phthalein, soluble in potash with a violet color, and dyeing, like brazil-wood, with iron and alumina mordants. Pyrocatechin, thus treated, gives a phthalein, soluble in potash with a transient blue color, analogous apparently to the coloring matter of logwood. Phloroglucin gives with phthalic oxide, a yellow phthalein. These new coloring matters contain (*a*) the phenol residue, and (*b*) the connecting or linking residue, which latter is supplied by the acid. Since the acid employed has but slight influence on the color, the former must be the color-forming constituent. Baeyer is of the opinion that many natural colors, especially those of the dye-woods, have this constitution; and hence that their synthesis may be looked for, when the nature of the linking body can be made out. This is a somewhat difficult matter, as in these colors the linking compound belongs to the sugar group or to the family of vegetable acids. Thus hæmatein—the coloring matter of logwood—which yields pyrogallol on fusion with potash, is a derivative either of that, of hydroquinone or of pyrocatechin. If its formula be $C_{16}H_{12}O_6$, the 4-carbon link present suggests that its synthesis may be effected by heating the above bodies with malic or succinic acids or with a derivative of crotonic acid.—*Ber. Berl. chem. Ges.*, iv, 658, July, 1871. G. F. B.

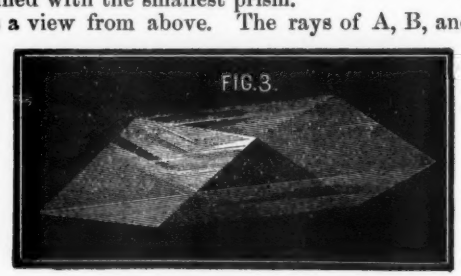
7. *A New Erecting Prism;* by JOSEPH ZENTMAYER.—Mr. Joseph Zentmayer exhibited and described a single prism, which erects the image completely, and in such a way that the incident and emerging rays are parallel, which, as far as we know, was never accomplished before. In connection with the microscope, as it was shown, it interfered very little with the definition, and, although the light is twice refracted and reflected, the loss of light is much less than one would expect. With the microscope, the prism is placed right above the objective, and the instrument may be used in any inclined position. A pair of such prisms might be used also for an erecting binocular microscope, of which the two bodies have the same inclination to the stage.

Fig. 1 shows the front and profile of the prism. The projection of the front is a square, that of the profile an isosceles triangle.



The angles at the base of the triangle are 27° 19′ for crown glass of a refracting index of 1·53, in order to obtain the greatest aperture combined with the smallest prism.

Fig. 2 is a view from above. The rays of A, B, and C of figs.



1 and 2 are the identical ones, their dotted parts are the projections of the rays inside of the glass, and their course may be read-

ily followed in the profile, fig. 1, where the upper ray, A, emerges as the lower one, and the lower ray, C, as the upper one.

As the ray A enters the perpendicular line above the lower edge, it will not be reflected out of its plane, while the rays B and C, entering the left side of the prism, reach the inclined faces, from which they are reflected to the opposite lower one, and are changed in their course to the right, from here again reflected, to emerge at the corresponding opposite point.

Fig. 3 is a perspective representation of the prism.—*Journal Franklin Institute.*

II. Geology and Natural History.

1. *On the Eozoon;* by Dr. Dawson.—Dr. Dawson published a reply to the first of the extended memoirs of Messrs. King and Rowney in volume 1 of the second series of the Proceedings of the Irish Academy. That our readers may have before them what is said by the original describer of the Eozoon, in opposition to the conclusions of the Irish investigators, we cite the following from his article.

In opposition to these facts, and to the careful deductions drawn from them, the authors of the paper under consideration maintain that the structures are mineral and crystalline. I believe that in the present state of science such an attempt to return to the doctrine of "plastic-force" as a mode of accounting for fossils would not be tolerated for a moment, were it not for the great antiquity and highly crystalline condition of the rocks in which the structures are found, which naturally create a prejudice against the idea of their being fossiliferous. That the authors themselves feel this is apparent from the slight manner in which they state the leading facts above given, and from their evident anxiety to restrict the question to the mode of occurrence of serpentine in limestone, and to ignore the specimens of Eozoon preserved under different mineral conditions.

With reference to the general form of Eozoon and its structure on the large scale, I would call attention to two admissions of the authors of the paper, which appear to me to be fatal to their case: First, they admit, at page 533 [Proceedings, vol. x], their "inability to explain satisfactorily" the alternating layers of carbonate of lime and other minerals in the typical specimens of Canadian Eozoon. They make a feeble attempt to establish an analogy between this and certain concentric concretionary layers; but the cases are clearly not parallel, and the laminæ of the Canadian Eozoon present connecting plates and columns not explicable on any concretionary hypothesis. If, however, they are unable to explain the lamellar structure alone, as it appeared to Logan in 1859, is it not rash to attempt to explain it away now, when certain minute internal structures, corresponding to what might have been expected on the hypothesis of its organic origin, are added to it? If I affirm that a certain mass is the trunk of a fossil

tree, and another asserts that it is a concretion, but professes to be unable to account for its form and its rings of growth, surely his case becomes very weak after I have made a slice of it, and have shown that it retains the structure of wood.

Next, they appear to admit that if specimens occur wholly composed of carbonate of lime their theory will fall to the ground. Now such specimens do exist. They treat the Tudor specimen with scepticism as probably "strings of segregated calcite." Since the account of that specimen was published, additional fragments have been collected, so that new slices have been prepared. I have examined these with care, and am prepared to affirm that the chambers in these specimens are filled with a dark-colored limestone not more crystalline than is usual in the Silurian rocks, and that the chamber-walls are composed of carbonate of lime, with the canals filled with the same material, except where the limestone filling the chambers has penetrated into parts of the larger ones. I should add that the stratigraphical researches of Mr. Vennor, of the Canadian Survey, have rendered it probable that the beds containing these fossils, though unconformably underlying the Lower Silurian, overlie the Lower Laurentian of the locality, and are, therefore, probably Upper Laurentian, or perhaps Huronian, so that the Tudor specimens may approach in age to Gümbel's Eozoon Bavaicum.*

Further, the authors of the paper have no right to object to our regarding the laminated specimens as "typical" Eozoon. If the question were as to *typical ophite*, the case would be different; but the question actually is as to certain well-defined forms which we regard as fossils, and allege to have organic structure on the small scale, as well as lamination on the large scale. We profess to account for the acervuline forms by the irregular growth at the surface of the organisms, and by the breaking of them into fragments confusedly intermingled in great thickness of limestone, just as fragments of corals occur in Paleozoic limestone; but we are under no obligation to accept irregular or disintegrated specimens as typical; and, when objectors reason from these fragments, we have a right to point to the more perfect examples. It would be easy to explain the loose cells of *Tetradium*, which characterize the Birds-eye limestone of the Lower Silurian of America, as crystalline structures; but a comparison with the unbroken masses of the same coral shows their true nature. I have for some time made the minute structure of Paleozoic limestones a special study, and have described some of them in the Trenton formation of Canada. I propose, shortly, to publish additional examples, showing fragments of various kinds of fossils preserved in these limestones, and recognizable only by the infiltration of their pores and other minute structures. I shall also be able to show that in many cases the crystallization of the carbonate of lime and the infiltration of

* Dr. Hunt, in a recent communication to this Journal for July, 1870, p. 85, is supposed to regard them as belonging to a great series of strata not hitherto clearly recognised, lying at the base of the Primordial, but distinct from and newer than the Upper Laurentian and the Huronian.

other substances have not interfered with the perfection of the most minute of these structures.

The fact that the chambers are usually filled with silicates is strangely regarded by the authors as an argument against the organic nature of Eozoon. One would think that the extreme frequency of siliceous fillings of the cavities of fossils, and even of siliceous replacement of their tissues, should have prevented the use of such an argument, without taking into account the opposite conclusions to be drawn from the various kinds of silicates found in the specimens, and from the modern filling of Foraminifera by hydrous silicates, as shown by Ehrenberg, Mantell, Carpenter, Bailey, and Pourtales.* Further, I have elsewhere shown that the loganite is proved by its texture to have been a fragmental substance, or at least filled with loose *débris*; that the Tudor specimens have the cavities filled with a sedimentary limestone, and that several fragmental specimens from Madoc are actually wholly calcareous. It is to be observed, however, that the wholly calcareous specimens present great difficulties to an observer; and I have no doubt that they are usually overlooked by collectors in consequence of their not being developed by weathering, or showing any obvious structure in fresh fractures.

With regard to the canal system, the authors persist in confusing the casts of it which occur in serpentine with "metaxite" concretions, and in likening them to dendritic crystallizations of silver, &c., and coralloidal forms of carbonate of lime. In answer to this, I think it quite sufficient to say that I fail to perceive the resemblance as other than very imperfect, imitative. I may add that the case is one of the occurrence of a canal structure in forms which on other grounds appear to be organic, while the concretionary forms referred to are produced under diverse conditions, none of them similar to those of which evidence appears in the specimens of Eozoon. With the singular theory of pseudomorphism, by means of which the authors now supplement their previous objections, I leave Dr. Hunt to deal.

With regard to the proper wall and its minute tubulation, the essential error of the authors consists in confounding it with fibrous and acicular crystals, and in maintaining that because the tubuli are sometimes apparently confused and confluent they must be inorganic. With regard to the first of these positions. I may repeat what I have stated in former papers—that the true cell-wall presents minute cylindrical processes traversing carbonate of lime, and usually nearly parallel to each other, and often slightly bulbose at the extremity. Fibrous serpentine, on the other hand, appears as angular crystals, closely packed together, while the numerous spicular crystals of siliceous minerals which often appear in metamorphic limestones, and may be developed by decalcification, appear as sharp angular needles usually radiating from centers or irregular disposed. Plate 44, fig. 10 (Ophite from Skye, King and Rowney's Paper, "Proc. R. I. A.," vol. x), is an eminent example of this; and whatever the nature of the crystals may be,

* "Quarterly Journal Geol. Society," 1864.

they have no appearance in the plate of being tubuli of Eozoon. I have very often shown microscopists and geologists the cell-wall along with veins of chrysotile and coatings of acicular crystals occuring in the same or similar limestones, and they have never failed at once to recognize the difference, especially under high powers.

I do not deny that the tubulation is often imperfectly preserved, and that in such cases the casts of the tubuli may appear to be glued together by concretions of mineral matter, or to be broken or imperfect. But this occurs in all fossils, and is familiar to any microscopist examining them. How difficult is it in many cases to detect the minute structure of Nummulites and other fossil Foraminifera? How often does a specimen of fossil wood present in one part distorted and confused fibers or mere crystals, with the remains of the wood forming phragmata between them, when in other parts it may show the most minute structures in perfect preservation? But who would use the disintegrated portions to invalidate the evidence of the parts better preserved? Yet this is precisely the argument of Professors King and Rowney, and which they have not hesitated in using in the case of a fossil so old as Eozoon, and so often compressed, crushed, and partly destroyed by mineralization.

I have in the above remarks confined myself to what I regard as absolutely essential by way of explanation and defence of the organic nature of Eozoon. It would be unprofitable to enter into the multitude of subordinate points raised by the authors, and their theory of mineral pseudomorphism as discussed by my friend Dr. Hunt; but I must say here that this theory ought, in my opinion, to afford to any chemist a strong presumption against the validity of their objections, especially since it confessedly does not account for all the facts, while requiring a most complicated series of unproved and improbable suppositions.

The last point which I shall mention is the taunt that so little further progress has been made in the investigation of Eozoon. With reference to this, I beg leave to doubt whether a process of confounding the actual structure of Eozoon with all manner of dendritic and crystalline forms, in the way followed by the authors, would constitute progress. But in so far as careful comparison with all specimens which have been recently found is concerned, some progress has been made; and I trust that it will soon be possible to bring forward not merely additional specimens illustrative of the structure of Eozoon, but fresh evidence of its wide geographical range, and also links of connexion with fossils of the Paleozoic rocks. The discovery recently made in Massachusetts, and alluded to by Messrs. Rowney and King, is itself not without importance. In the meantime I am content to accept the investigations of Messrs. King and Rowney as nearly exhaustive of the natural history of those imitative forms which may be confounded with Eozoon, and therefore as in a certain way useful in the further prosecution of the subject. As already stated, I am at this

moment engaged in following out, as opportunity offers, two lines of investigation bearing on the following points:—(1) the study of the Lower Silurian and Primordial successors of Eozoon, and (2) that of the tubulation and other structures similar to those of Eozoon preserved in the Paleozoic rocks.

2. *Discovery of a Large Bone Cave in Bavaria.*—During the cutting of the railway from Nuremberg to Regensburg by the Bavarian Eastern Railway Co., it was necessary to cut directly through a piece of mountain chain in Schelmengraben near Regensburg. It was owing to this that this bone cave was discovered, and its miscellaneous contents were able to be examined and arranged. Since the railway cut right across the middle of the cave, it allowed it to be very thoroughly examined, and under the most favourable circumstances and in daylight, as has been the case in very few other instances. The railway company have given every facility in their power that the cave should be thoroughly examined, and under the direction of Profs. Fraas and Zittel, a gang of men were actively employed for many days, and the objects so obtained were carefully preserved. From the local German papers the following particulars have been obtained, which, allowing for a little local coloring and exaggeration, show the find to have been a most important one, and one that may well come under the notice of the International Congress of Archæology and Anthropology at their meeting this year, where the whole question of bone caves and their contents is to form a prominent subject for discussion.

The cave in question was originally, when first discovered about two years ago, 28 metres (about 91 ft.) long, and was simply a fissure in the Jura limestone, which had been enlarged by running water. Its opening was visible half way up the mountain side, partly hidden in dense woods. It stretched from north to south, with a slight turn toward the west of about 15°. The new line of railway cut deeply into the hill side, and during the course of this year has already cut away one half of the cave; but unfortunately the contents were employed on the line. On this account, only the part not touched was able to be excavated and examined, and this was 11 metres (36 ft.) long, 2 metres (6½ ft.) wide, and in the middle 3 metres (9½ ft.) deep. Wood ashes and pieces of coal, together with pieces of pottery, had accumulated to about the height of three feet, in the midst of which were sharp splinters of flint, and a thick mass of broken and split bones, and the shattered skulls and jaw bones of a heterogeneous mass of animals of all kinds. In the lowest layer no trace of men, either by their remains or by their handiwork, could be found; all the remains consisted of bones of animals, chiefly the cave bear, hyena, and lion. These cave-dwelling animals appear to have been the first and earliest possessors of the cave. But soon after this, men must have discovered the cave and inhabited it, for, from this layer up to the newest layer of all, the presence of man is clearly shown, and the remains of their feasts and of their daily life are mingled with

those of the previously-named animals. The most numerous remains consist of flints, of which many thousand were found; but these do not appear to have been used as implements, but come rather under the category of flint-flakes, the chippings from knives, saws, lances, &c. The most perfect one found is three inches long and half-an-inch wide, and is toothed like a saw, and was probably used as such to saw off the ends of the deer's horns, of which quantities were found.

In order to judge of the age in which men began to inhabit this cave, we must examine the remains of the bones and skeletons of the animals which they hunted, and whose flesh was eaten in the cave. The most conspicuous among these is the cave bear, and although it might at first sight appear very difficult to recognize in the broken and burnt bits of bone that they really do belong to the cave bear, nevertheless, careful comparison with specimens in museums has proved that this is the case. Every care seems to have been made to utilize to the utmost all parts of this animal, which was apparently the most important game in the surrounding forests, and which no doubt required much labor and time to capture. At the same time, together with the bones of the cave bear are found bones of the elephant and of the rhinoceros, but not many in comparison. These remains, however, show conclusively, by the way in which they have been spilt up and broken, that man hunted these animals at the time he first appears on the scene. Remains of horses, oxen, cats, and wolves were also met with, and in proof that the early inhabitants were not unmindful of fish, there are the bones and scales of large pike and carp. The smaller bones of mice and frogs do not appear to owe their origin so much to man as to the owls, which seem to have held possession of the cave as well.

Great interest attaches to the fragments of pottery which were found in the cave, and which rival the flint flakes in quantity. It appears to have been all hand made, but although rough, shows considerable beauty of shape and form. It is possible to put together from the fragments one or two more or less complete vessels, which, however, show great diversity as to size, &c., some being between 10 and 20 centimetres in diameter. The material of which they are made appears to be clay mixed with sand, but few, if any, seem to have been regularly burnt. Much of the pottery is ornamented with lines or rows of dots, which run in zigzag lines over the wider parts. The internal smoothness would appear to be due to the river mussel, Unio, obtained from the river Naab, which flows close by, and of which many well rubbed and polished specimens were found in the cave. A block of granite with one side rubbed smooth by long usage, and appearing quite polished, can hardly be anything else than a well-worn millstone, and this is rendered more probable by two holes having been bored into the upper side as if for the purpose of affixing a handle. The presence of this millstone would indicate the cultivation of land in the immediate neighborhood, which is confirmed by the finding of several spindles made of clay.

The different objects found in this cave are of great interest, as they apparently run counter to the somewhat hard and fast lines which have been drawn as to different well marked periods in the early history of man.—*Nature*, May 30.

3. *Pseudomorphs of Serpentine with the form of Staurolite;* T. D. Rand (Proc. Acad. Nat. Sci. Philad., 1871).—At the line between Philadelphia and Montgomery counties, the well-known steatite bed, beginning on the west side of Chestnut Hill, about three miles distant, crosses the Schuykill and continues in a nearly southwest by south direction (exactly S. 54 W.), beyond that river about two miles and a half, where it crosses the valley of Mill Creek, and ends, or sinks beneath the surface. Perhaps the most conspicuous and remarkable rock of this belt is a steatite, containing a black serpentine. This rock in many places projects above the surface of the ground in immense masses, particularly at Mill Creek, seeming to have resisted erosion and decomposition to a remarkable degree. It lies on the northwest side of the steatite proper. The whole aspect of this curious formation suggests a rock originally containing crystals of some mineral, but metamorphosed almost beyond recognition. To this mineral I believe no clue has heretofore been obtained. Nearly all these black masses, which vary in size from a half-inch or less to several inches, are irregular in form, and adhere so closely to the matrix that sections only can be obtained, which, however, rarely show any angles, or other than a nodular form, so that the rock has by some been considered a conglomerate. Even in weathering, the two, except in one place hereafter mentioned, seem to weather so much alike that no clue to the form can thus be had.

About two years ago, however, I found, near the soapstone quarry, on the northeast bank of the Schuykill, one of these serpentine masses presenting a stellated form of six rays, or of one large crystal crossed by two smaller at angles of about 60 and 120 on the section plane, suggesting staurolite. A few days ago, while with Prof. E. J. Houston examining this rock on the southwest side of Mill Creek, a piece was found containing a broken crystal $4\frac{1}{2}$ inches $\times$ $1\frac{1}{2}$ $\times$ $1\frac{1}{2}$, presenting two well-defined sides, and upon a cross-fracture these sides were found to continue to another forming with one of the sides an angle of about 75° on the section, which approximated a plane at right angles to the faces. The steatite in which it was imbedded, and the serpentine itself, contained ferruginous dolomite or breunnerite.

On the northeast side of Mill Creek, a portion of the rock in place was found very much weathered on the surface, the steatite being cavernous and decomposed, and very soft and brittle, owing, probably, to a large admixture of ferruginous dolomite, but the serpentine gone entirely, save a little pulverulent oxide of iron; the cavities were nearly all lenticular in shape, but too regular to be other than matrices of crystals, while in two cases distinct cruciform cavities with angles of about 60° were observed. The portions of rock containing these were cut out, and in

one of them lead was poured, and a cast obtained, which, while irregular and rough, was a fac-simile in metal of the common cruciform twins of staurolite. Portions of the same rock, which had not altered, were found containing the serpentine in distinct crystals, irregular in outline, but twinned at angles of about 60°.

4. *Hisingerite, from the Gap Mine, Lancaster County, Pa.;* T. D. RAND.—Black amorphous; lustre between resinous and vitreous; streak, brown. Fracture conchoidal, brittle. H.=2½–3. G.= 2.11.

Analysis, omitting 1.13 per cent. gangue:—

Water at 212°	14.30	
at redness	9.89	24.19
Silica		35.40
FeO		12.53
Fe_2O_2		27.46
		99.58

In a cutting through decomposed mica schists, on the new line of the Philadelphia, Wilmington and Baltimore Railroad, about half a mile southwest of Gray's Ferry, there is a white efflorescence, alkaline to the taste. It consists chiefly of sulphate of soda, an unlooked-for mineral in such location.—*Proc. Acad. Nat. Sci. Philad.*, 1871.

5. *Descriptions of new species of Fossils from the vicinity of Louisville, Ky., and the Falls of the Ohio;* by JAMES HALL and R. P. WHITFIELD. 7 pp. 8vo. Published May, 1872, in advance of the Report on the State Museum. Contains descriptions of species of Orthis, Spirifera, Pentamerus, Aviculopecten, Yoldia?, Nucula, Cypricardinia, Polyphemopsis, and of the new genus Ptychodesma, based on a modioloid shell.

6. *Mineralogical investigations of vom Rath.*—The 144th volume of Poggendorff's Annalen contains a continuation of the valuable mineralogical and crystallographic researches of vom Rath, treating of the composition of the lime-soda feldspars; composition of orthoclase; the ersbyite of Pargas; sahlite in the Pennine Alps; wollastonite of Mt. Somma; allophane of Dehrn in Nassau.

7. *Proceedings and Transactions of the Nova Scotian Institute of Natural Science of Halifax, Nova Scotia.*—Part I. of vol. iii, 94 pp. 8vo. (5*s.*), has recently been issued. It contains several papers on the geology, natural history, and meteorology of Nova Scotia.

8. *Marc Micheli; On some Recent Researches in Vegetable Physiology.* An article in the *Archives des Sciences* of the *Bibliothèque Universelle* of Geneva, in October last, reproduced in English in the Ann. and Mag. of Natural History, London, for February and March last.—Micheli is the translator into the French of Sachs' volume upon Vegetable Physiology, and we trust he will translate the other volumes of the series to which this belongs. The researches which are first considered in this inter-

esting article, relating to the movements of chlorophyl grains in the cells of leaves under the light, have been noticed in this Journal. The discovery that the green of leaves is less intense under direct sunshine than under diffuse daylight is attributed to Sachs. The discovery of the movement of the grains toward the light, that they group themselves during the day upon the more illuminated horizontal walls—and this only under the action of the more refrangible solar rays—and retreat at night to the perpendicular walls, belongs to Famintzin. Next Borodin (as we have already recorded), explained Sachs' observation by ascertaining that direct sunlight when too ardent, caused the same retreating of the green grains that darkness does. Then Rose remarked that the movement of the green grains involved that of the whole protoplasmic mass; and Frank (in Bot. Zeit.) finds that the result of prolonged unilateral illumination is to accumulate the grains in the more strongly illuminated side of the cell,—that, like zoosposes, they incline to seek the light. In these, as in all periodical movements, currents of protoplasm, and in heliotropic curvatures, the most refrangible rays only are efficient. As to the action of light in assimilation, the decomposition of carbonic acid, and the formation of chlorophyl, the net results of all recent researches, those of Famintzin, Krauss, Prillenx, Boronetsky (Bot. Zeit., 1871, No. 13), and Pfeffer, go to confirm the now well-settled view that these phenomena are dependent solely upon luminous intensity. Dr. Pfeffer, by very complete lines of investigation (which we have no room to detail), made out that if

White light decomposes		100	parts of carbonic acid,
Red and orange	"	32.1	"
Yellow	"	46.1	"
Green	"	15.0	"
Blue, indigo, and violet	"	7.6	"

The curve of assimilation, nearly parallel to the curve of luminous intensity, culminates between the Fraunhofer lines D and E.

As to phenomena which result from the absence of light, Krauss has studied the difference between stems and leaves when subject to etiolation; the limbs of leaves undergoing complete arrest of development in darkness, while the internodes of stems elongate far beyond normal dimensions. The blade of a leaf, it appears, completes its growth after coming into the light solely from the materials which it assimilates (into starch or its equivalent); starch stored up in the older tissues is of no use to it. In darkness none of this is produced, and so its growth is arrested. The exaggerated length of internodes is due to very different causes, and is related to the phenomena of tension, which always intervene in stems between the medulla, or active part, on the one hand, and the ligneous and cortical cells, or passive parts, on the other.

"From an anatomical point of view the etiolated internodes are distinguished by presenting all the characters of very young inter-

nodes just issuing from the bud; the thickening of the walls of the ligneous and cortical cells, which characterizes adult stems is here wholly absent. This thickening, indeed, is related by bonds which are not yet very exactly understood to the presence of leaves on the internode. In darkness the leaves not being developed, the cells retain the primitive thinness of their membranes.

"This being understood, the elongation of the etiolated stems is easily explained, thanks to the intervention of two factors. In normal stems the medulla has always a tendency to elongate; it is the peripheral layers that arrest it; in young stems these are subjected to a tension strong enough to cause them to shorten considerably when they are isolated. But in proportion as their walls become thickened the resistance becomes more effective, and we see this in the fact that their contraction, when they are separated from the rest, becomes less and less. In darkness their walls do not thicken, and nothing is opposed to the elongation of the medullary cells. This is the first factor.

"With regard to the pith itself, M. Krauss has already shown, in a former work (Botan. Zeit., 1867, Nos. 17, 18), that it has the property of elongation solely by the interposition of aqueous molecules between the cellulose molecules. This interposition may take place in the etiolated as in the normal plant; the pith is, therefore, the only part of the plant which continues to *grow* actively in the dark. This growth is precisely the second factor of the elongation of the internodes; and by combining it with the absence of resistance in the peripheral layers, we can understand that considerable results may be produced."

The recent observations which relate to the action of cold upon plants, notably Gœppert's paper, have been already referred to in our pages, but Micheli's abstract of Schrœder's researches upon the "Spring Period of the Maple" we will reproduce:—

"The author has paid attention to all the successive phases presented by the development of the vegetation, from the ascent of the sap to the moment when the expanded leaves begin to decompose carbonic acid. This is one of those complete and conscientious works which, even when they do not contain any very novel results, are, nevertheless, very useful to read and consult; but it is difficult to give a clear notion of them in a few words. A glance at the course pursued by M. Schrœder will show the great number of facts which group themselves within a framework such as he has adopted.

"The first part is entirely devoted to the study of the sap, its ascent and its composition. The maple, under the latitude of Breslau, "*weeps*" for about a month; the sap rises gradually to a certain level, whence it descends again by degrees, in proportion as the development advances. Holes pierced in the trunk, at different heights, enabled this sap to be collected daily; and very numerous analyses keep us informed of the smallest variation in its composition. It always contains sugar, a transitory product of the transformation of the starch accumulated in the tissues

during the preceding summer, and destined to become re-transformed when it reaches the buds. The proportion, faithfully represented by a great number of curves, is but slight, at the first awakening of vegetation; it increases gradually up to a certain maximum, in proportion as the vital phenomena acquire more intensity; and, finally, it diminishes when the young organs, approaching the term of their development, are on the verge of sufficing for themselves. These facts are, therefore, perfectly in accordance with such a theory of growth as has been established by the researches of modern observers. The albumen and the mineral salts are successively studied from the same point of view; and their dissemination in the sap, at different heights at the same moment, and at different periods, is exactly governed by the different phases of development. The second part is devoted to the microscopic examination of the bud. The different substances which are called upon to assist in the development of the young leaf are traced by means of reagents from cell to cell. Two, especially, give origin to detailed observations, namely starch and tannin. The dissemination of the former in the different tissues, its transportation through the starchy layers of the fibro-vascular bundles, its disappearance toward the point of vegetation, at the surface of which it speedily reappears as cellulose—all these different phases are taken up step by step; and here, again, we find a confirmation of all that theory led us to foresee. As to tannin, it is developed in all the cells of the bud; and when once it has made its appearance it persists there, without appreciable change. Its function has greatly embarrassed M. Schrœder, as he was unable to recognise in it any of the characters of an excrementitial product, properly so called. The fact that it is constantly to be found in the youngest tissues (in which life is most intense), seems to indicate that it is a sort of final product, charged with a still unknown office in the life of the cell. If the true chemical nature of this substance were better known, the solution of the problem would, perhaps, become easier." A. G.

9. *Botany for Beginners; an Introduction to the Study of Plants*, by MAXWELL T. MASTERS, M.D., F.R.S. London, 1872: Bradbury, Evans & Co. Pp. 185, 18mo. A series of articles treating of elementary botany with admirable freshness and clearness, and illustrated by wood-cuts of uncommon excellence, attracted our attention during the past year in the pages of successive numbers of the Gardeners' Chronicle. These are gathered into this little volume. The articles, it appears, were from the pen of Dr. Masters; the illustrations were contributed by Mr. Worthington Smith. There are ten chapters, or lessons. The first, explaining how to begin, and starting with early spring flowers, is a study of a willow and poplar, followed by the ash and elm. The second, tulip and hyacinth. The third, the apple and cherry, followed by the lilac compared with the ash, and so on. At the end are short chapters on fruit and seeds; on seedling plants and growth; on classification, description, and points to be looked to in each organ; and finally, a particularly good one on plant life.

The book is truly admirable in plan and execution,—especially so for the skill with which the main points are chosen and handled, and less relevant matter passed by. There is not a particle of rubbish from old books; but room is found for some notice of phyllotaxy, a view of insect-fertilization in orchids, and a fair account of natural selection. The natural classification of plants is characterized as an attempt to determine their degree of relationship, and to ascertain their lineage. A. G.

We learn that Dr. ROBERT WRIGHT, the distinguished Indian botanist, recently died at Granby Lodge, his residence in England since he returned from active service.

G. F. REUTER, the curator for many years past of the herbarium of M. Boisner, and an excellent botanist, died in June at Geneva. A. G.

10. *Musci Appalachiani: or specimens of Mosses collected mostly in the eastern part of North America;* by COE F. AUSTIN, Closter, New Jersey, 1870.—To the students of bryology no auxiliary is more important than a collection of well-prepared and reliably-named specimens. Such a collection Mr. Austin has given us under the title above cited; and it comes quite opportunely, as those of a similar kind, relating to North American bryology, are now with difficulty obtained.

The *Musci Appalachiani* are put up in uniform sets, each set consisting of copious and excellent specimens of 450 species and varieties, fastened on white paper of suitable sizes, and arranged in proper order within folio sheets of strong brown paper, the whole secured between two thick paper boards. To each species and variety is attached a printed label giving the name, the necessary synonyms, the habitat, and in the case of a new species, a sufficient Latin character. A title-page, and complete index, together with a separate pamphlet containing the labels, accompany each set.

The specimens, with very few exceptions, were collected by Mr. Austin in New Jersey—a state representing more fully, perhaps, than any other, our bryological flora east of the Mississippi river. Its northern portion, being traversed by ranges of the Appalachian mountains, furnishes the species peculiar to such stations. In its central portions are found the species of the middle and western States; and on its eastern and southern borders occur in "The Pines," so-called, species ranging south on the Atlantic coast as far as Florida and even beyond to the West Indies. This latter portion of the State is remarkably rich in species of Sphagnum, fine and abundant specimens of which form a special feature of the collection.

It is gratifying to find, as is shown by the collection, that within the limits indicated in the title, large additions have recently been made in new species, and no species which, though previously known, have not heretofore been detected in this country. For these additions to no one are we so much indebted as to Mr. Austin himself. Among them may be mentioned—*Sphagnum Portoricense*, *Sph. Austini*, *Sph. fim-*

briatum, Sph. Girgensohnii, Sph. teres, Sph. Pylaesii (in fruit), *Sph. cyclophyllum* (in fruit), *Sph. neglectum, Sph. laricinum, Sph. Wulfianum, Sph. Lindbergii; Micromitrium Austini, Micr. synoicum, Micr. megalosporum; Ephemorum papillosum; Anoectangium Peckii; Conomitrium Hallianum; Pottia riparia; Didymodon cylindricus; Didym. diversifolius; Desmatodon Porteri; Orthotrichum Porteri, Orthot. Peckii, Orthot. Lescurii, Orthot. sordidum, Orthot. Ohioense, Orthot. citrinum; Bryum cyclophyllum; Fontinalis Sullivanti. Font. filiformis; Omalia gracilis; Leskea Austinii; Rhynchostegium geophilum; Plagiothecium Mullerianum, Plag. Passaicense, Plag. latebricola, Plag. turfaceum, Plag. subfalcatum; Hypnum Bergense, Hyp. Closteri, Hyp. Novae-Caesareae.* The *Musci Appalachiani* can be had of Mr. Austin at the exceedingly low price of twenty-five dollars per set. His address is Closter, New Jersey. W. S. S.

11. *Gray's Botanical Series. Botany for Young People*, Part II. *How Plants behave;* by Asa Gray. 46 pp. 16mo, with 40 wood-cuts. New York. 1872. (Ivison, Blakeman, Taylor & Co.) —This little volume for young people has been made every way attractive:—in its tinted paper and excellent printing by the publishers, but more, in its simple and interesting account of the habits of certain plants, by the author. The special subjects of the chapters are: I, How plants move, climb and take positions; II, How plants employ insects to work for them; How certain plants capture insects.

12. *Journal of Zoology.*—A new Zoological Journal (Journal de Géologie) has been commenced in Paris by Prof. Paul Gervais of the Muséum de Paris. Its first number appeared in January of the current year, and it will be issued every other month. Each number is to contain 5 or 6 sheets and 4 or 5 plates. Those desiring it should address M. Arthur Bertrand, 12, rue Hautefeuille, Paris. Price for subscribers in foreign countries, 30 francs a year.

13. *Zoölogie et Paléontologie Générales, par M. Paul Gervais.* —Under this title, 5 numbers have been issued of a work on Fossil Vertebrates and their relation to living species. It will be completed in about 13 numbers, containing each 3 sheets of text, and 4 lithographic plates, costing each 5 francs.

14. *Rectification of T. A. Conrad's "Synopsis of the Family of Naiades of North America"*; by Isaac Lea, L.L.D. 46 pp., 8vo. Philadelphia, 1872.—A new edition, originally from the Proceedings of the Acad. of Nat. Sci. of Philadelphia, Feb., 1853.

III. Astronomy.

1. *The Double Star Castor.*—In the *Astronomical Notices* for May, Mr. Wilson advances the somewhat startling idea that the components of the double star castor are describing an hyperbolic orbit. The observations since 1740, when plotted, give an apparent orbit of eccentricity 2·2. The real orbit, he claims, has an eccentricity 3·16. The recent observations show very decidedly the necessity of extending the periodic time of the orbit, even if

Mr. Wilson's views be not confirmed. The periods given by Sir John Herschel and Admiral Smyth were 253 and 240 years, but Mr. Hind obtained in 1845 a period of 632 years, and Captain Jacob, in 1846, a period of 653 years. The observations since 1845 seem to require a still further extension of the period, and may require us to accept Mr. Wilson's remarkable conclusions.

2. *Meteorite of Ibbenbühren, Westphalia;* G. Rose.—This meteorite fell on the 17th of June, 1870. It is peculiar in consisting of but a single mineral, and that bronzite, composed of silica 54·51, protoxide of iron 17·53, protoxide of manganese 0·29, magnesia 26·43, lime 1·04, alumina 1·26=101·06. The specific gravity of the mass was 3·405 to 3·404, and that of the crystalline grains of bronzite 3·428 and 3·425.

Rose remarks that one other meteorite, that of Manegaum, investigated by Maskelyne, has essentially the same constitution (Si 55·70, Fe 20·54, Mg 22·80, Ca 1·32=100·36); while two others consist of a single mineral only, the Chassigny being made up of olivine alone, and the Bishopville of enstatite.

The Ibbenbühren mass is somewhat ovoidal, has a black smooth exterior rind, showing evidences of fusion. The length is nearly five inches (one-eighth of a meter).

3. *Meteorites of India.*—Tschermak has described two Indian meteorites. One fell at Shergotty on the 25th of August, 1865, and closely resembles those of Stannern (1807). It consists of augite, a colorless tesseral silicate of the composition of labradorrite which has been named *maskelynite*, and magnetite. This is the first mention of the latter two species in a meteorite. The other fell near Gopalpoor on May 23, 1865. It consists of nickeliferous iron, magnetic pyrites, chromite, chrysolite, bronzite, and a feldspar-like substance.—*Akad. Wiss. Wien*, Feb., 1872.

IV. Miscellaneous Scientific Intelligence.

1. *Cause of the blue and violet chatoyant colors of Fishes.*—G. Pouchet finds that there is a constant anatomic cause for the bluish or violet chatoyant color of some fishes. There is under the skin a layer of small ovoidal or irregularly spherical granules, yellow by transmitted light, which is the source of the color, this color being a complementary one to the yellow. The diameter of these *irising* granules varies from 2 to 4 or 5 thousandths of a millimeter; and each is formed of a pile of extraordinarily thin lamellæ, applied one to the other, but separately distinguishable under the microscope. Pouchet concludes that the color is due to a kind of fluorescence.—*Acad. Sci., Paris, May, Les Mondes, June* 6.

2. *Iron in the blood.*—Boussingault finds the amount of metallic iron in aliments as follows: the minimum, in carots, 0·0009 gram; the maximum, in the blood of hogs, 0·0534; in beer, ·0040; in the wine of Beaujolais ·0109. The ration of a French sailor in the marine service contains 0·0661 gr.; of a French soldier, 0·0780 gr.; of an English workman, 0·0912 gr.; of an Irish workman, 0·1000 gr.;

of a horse of the reserve cavalry, 1·0166 gr.; of a cow, 1·365 gr. In vertebrates the quantity of iron does not exceed a thousandth of the weight; in invertebrates, probably not four ten-thousandths. It is usual to attribute the red color of the blood to the presence of iron. Yet the white blood of invertebrates contains almost as much iron as the red of vertebrates. Also, plants not green, like mushrooms, contain as much iron as the green plants. Boussingault concludes that of all substances the blood is that which contains the largest amount of iron, and of assimilable iron, since it has already been assimilated.—*Acad. Sci. Paris, May, Les Mondes, June* 6.

3. *Prismatic bows on the surface of the Lake of Geneva.*—On February 11, between two and three o'clock, M. Elie Wartman observed two concentric bows with the colors of the rainbow on the surface of Lake Geneva, and during part of the time a third bow. He attributes the phenomenon to the presence of numberless particles floating on the surface of the lake. He remarks that early in February the wind from the southwest carried over the lake immense quantities of fine particles, mixed with smoke from the many chimneys of the city of Geneva, and that these materials, arrested by the severe storm which continued for several days over the country, were distributed over the waters with remarkable regularity; and so vast was the quantity, that the current of the Rhone took several days to carry it off.—*L'Institut, June* 5.

4. *Chesnut tree* (*Castanea vesca*).—Mr. C. D'ETTINGSHAUSEN remarks that a species of Castanea was common in the Tertiary flora of Leoben (Styria), and its leaves present a series of variations in form not observed in other species, but that the same have recently been met with in the *Castanea vesca* of the present time, and hence he concludes that the latter is a descendant from the former.—*Akad. Wiss. Wien*, Feb., 1872.

5. *Ueber krystallinischen Hagel im thrialethischen Gebirge, und über die Abhängigkeit der Hydrometeore von der Physik des Bodens;* von H. ABICH. 260 pp. 8vo, with 5 plates and a chart. Tiflis (Caucasus), 1871.—This large volume on the crystalline hail of the Thrialeth mountains in the Caucasus, is a part of a work entitled "Materialen zu einer Klimatologie des Kaukasus," issued under the direction of A. Moritz, Director of the Observatory at Tiflis. The work has more than a meteorological value, for it treats of the orography of the mountain region of Thrialeth and of the volcanic Meridian mountains, devoting over twenty pages to these subjects. It then describes a large number of hail storms that occurred in the region, giving careful statements as to the attending phenomena, and the special peculiarities of the storms in different parts of the Caucasus, and discussing the theories of the origin of hail. Prof. Abich is an eminent geologist as well as physicist, and has here made the most important contribution to the science of hail that has appeared. Of the figures of hailstones, those of plates 2 and 3 are of very remarkable form. Most of them have a central portion, 1¼ to 1½ inches in diameter, radiated outward in structure, but divided into six segments by

radiating planes; and over the exterior large angular prismatic crystals stand out that are half an inch and larger in diameter, and in some cases an inch or more long.

6. *Die Wirbelstürme, Tornadoes und Wettersaülen in der Erd-Atmosphäre, mit Berücksichtigung der Stürme in der Sonnen-Atmosphäre; dargestellt und wissenschaftlich erklärt* von Dr. THEODOR REYE, Prof. Univ. Strasburg. 248 pp. 8vo, with 4 storm-charts and 30 woodcuts and lithographs. Hanover. 1872. (Carl Rümpler.)—In this work Professor Reye has treated the subject of whirlwinds, tornadoes and waterspouts with great fulness and system. He presents the facts at length, discusses freely the various theories, and gives the practical rules which flow from the observations hitherto made. He does full justice to Redfield's views and observations, quotes largely from his papers, copying some of his charts; and also cites freely from the papers of Professor Loomis, Reid and Piddington, and from various European sources. He adopts the idea of cyclones, and illustrates the earth's cyclones by descriptions, and a fine plate illustrating the cyclones in the sun's atmosphere. The frontispiece of the work is a copy of Olmsted's plate of "Whirlwinds from the burning of a canebrake," from II, vol. xi, of this Journal (1851). The work closes with a list of books and articles on the subject of which it treats.

7. *American Journal of Conchology;* GEORGE W. TRYON, Jr., Editor.—The cover of the 4th number of vol. vii. announces that the publication of this journal will be therewith discontinued. It has done good service both to zoölogy and geology, and we greatly regret for the sake of American science that it cannot be sustained. The last number, like very many of those preceding it, contains excellent colored plates of shells; in this case illustrating an article by A. J. Garrett of Tahiti, on Feejee fresh water and terrestrial gasteropods. The papers hereafter presented will be published by the Academy of Natural Sciences, to whose conchological committee its subscription list has been transferred.

8. *Notes sur les Tremblements de Terre en* 1869, *avec Supplements pour les années antérieures de* 1843 *à* 1868, par M. ALEXIS PERREY, Prof. Hon. à la Faculté des Sciences de Dijon. From *Mem. Cour.* of the Belgian Academy for 1872.—Prof. Perrey, the indefatigable investigator of earthquakes, here gives a review of the facts connected with the earthquakes of 1869, besides adding to his collection of facts pertaining to previous years.

9. *Half-Hour Recreations in Science*, published in numbers of 20 to 60 pages, by Lee & Shepard, Boston. Price of each, 25 cts. —The first number of the series contains Proctor's excellent paper on "Strange Discoveries respecting the Aurora and Recent Solar Researches, and the second is an instructive lecture by Prof. R. Virchow on "The Cranial Affinities of Man and the Ape. The others out, or announced as in progress, are of equal interest, and give full assurance that the series will be a valuable addition to any library. The paper and printing are excellent.